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A NEW CONTRIBUTION TO THE EXPERIMENTAL STUDY OF THE SIZE-WEIGHT ILLUSION

BY

R. NYSSEN AND J. BOURDON,

(Brussels)

1. Historical Introduction

If one lifts simultaneously or successively two objects of the same weight, but of different sizes, the smaller seems to be the heavier. Since the description of this illusion by Charpentier in 1891, several authors have studied the different aspects of the phenomenon by trying to determine:

(1) the mechanism of the illusion (Charpentier, Claparède, Flournoy, Van Biervliet, Demoor, Ley, Seashore, Koseleff, Huang);

(2) the frequency of the illusion (Claparède, Demoor, Rey, Ciampi, Nyssen and Bourdon);

(3) the magnitude of the illusion (Dalhem, Ley, Rey, Nyssen and Bourdon).

From these researches it results that: 1) this size-weight illusion would be more or less precociously acquired during childhood; 2) its frequency is generally very high, but may vary according to the technique applied; 3) its magnitude also depends on the technique used (Ley); it might also depend on the development of the intelligence (Rey).

Following the studies of Demoor upon mental deficients, the size-weight illusion has been examined in its applications in the field of psychopathology and especially upon mentally deficient children by Claparède, Ley, Dalhem, Rey, Ciampi. The observations of Demoor, according to which the illusion would be absent or inversed in a more or less important number of profound mental deficients, are known. Less numerous are the researches on the phenomenon in oligophrenic or dement adults (Claparède, Ciampi, Nyssen and Bourdon). We have examined this problem in a former study (1955).

2. Aim of the Present Study

In order to make a comparative study more extended than our former

study (1953) in normal adults on the one hand, and mental deficients on the other hand, we have intended to examine in normal subjects:

(1) the frequency of the phenomenon of the size-weight illusion,

(2) its magnitude,

(3) various factors which might influence its frequency and its magnitude,

(4) its essential cause.

3. Instrumentation and Techniques

We have used a series of 14 parallelepipeds in beechwood.[1] One of our blocks was manifestly smaller (less high) than the 13 others: it was 21 cm. long, 5 cm. broad and 2 cm. high. The 13 other blocks were all 21 cm. long, 5 cm. broad and 7 cm. high. The differences of absolute weight had been determined by the introduction of iron filings in the blocks. The small block and the lightest one of the series of 13 large blocks each weighed 500 gr. The 12 other large blocks weighed respectively 550, 600, 650, 700, 750, 800, 850, 900, 950, 1000, 1050 and 1100 gr.

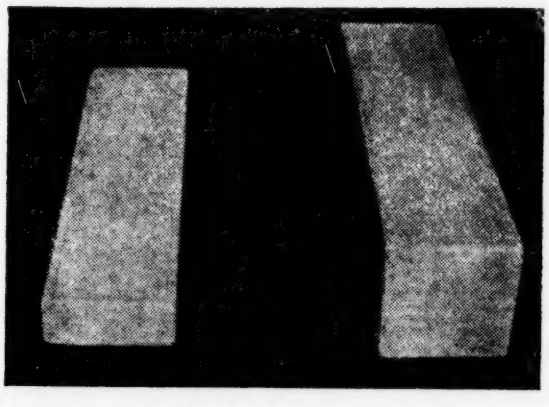


Fig. I. The small block and one of the larger ones.

The liftings have been done by all our subjects in identical conditions: the subject was standing near a table upon which were lying in front of him the two weights which he had to compare, the larger one on his left hand side, the smaller on his right hand side, at a distance of 15 cm. from each other, their longitudinal axis pointing to the subject. The subject had invariably and successively to lift each block by grasping it between his thumb and the four other fingers of his right hand. Each one of the 13 large blocks had to be lifted and its weight compared every

[1] Parallelepipeds in wood were used among several other forms by P. Koseleff, for the analytical study of the phenomenon of the size-weight illusion in normal man.

time with the one of the little block. Preliminary researches done upon two groups consisting each of 100 normal and adult subjects had taught us that the order in which the two comparative liftings were made (large-small or small-large) has practically no influence upon the frequency and the magnitude of the illusion. So, we have invariably had the big block lifted before the little one in all our researches with normal subjects. We indicated to the subject successively the two blocks he was to lift, without however referring to the difference of size.

4. Frequency of the Size-Weight Illusion

By applying three methods, differing from ours, upon 50 normal adults, Ciampi has found a degree of constancy of the illusion which is far lower than the frequency we have recorded. On the other hand, Claparède had already found the illusion present in 93 % of the pedagogically backward children of the special classes in Geneva, and A. Doll in 100 % of mentally abnormal children with a mental age of 8 years. In a group of 31 normal children, from 7 to 14 years old, A. Rey has found three times the absence of illusion, but the author indicates that it concerned "very feeble pupils of a feeble class".

We have examined the frequency of the illusion in 600 normal adult subjects divided into three groups, according to the different experimental conditions under which we have worked:

(1) 300 subjects have been asked to compare B. 500 gr. (large block) with b. 500 gr. (little block) and to say which of both weights was heavier;

(2) 200 subjects have been told to indicate the heavier block of each pair of the series: B. 800—b. 500, B. 750—b. 500, B. 700—b. 500, etc. until B. 500—b. 500;

(3) 100 subjects have been told to indicate the heavier block of every

TABLE I

Groups	Number of subjects	Presence of illusion	Absence of illusion	Inversion of illusion
I	300	99 %	0,66 %	0,33 %
II	200	99 %	1 %	0 %
III	100	100 %	0 %	0 %

pair of the series: B. 1100—b. 500, B. 1050—b. 500, B. 1000—b. 500, etc. until B. 500—b. 500.

The second and third groups have been conceived in order to examine the influence of a long series of comparisons, during which the large block has the highest weight every time, upon the frequency of the illusion when comparing the blocks B. 500 and b. 500.

So we observe that, under the different experimental conditions we have described above, the frequency of the illusion is practically the same. In the whole of the 600 subjects, the illusion is present in 99.17 % of the cases.

5. Magnitude of the Size-Weight Illusion

Like A. Ley and A. Rey, we are of opinion that the magnitude of the illusion can be measured in the following way: while the weight of the small size remains constant, the weight of the large size is regularly modified. It is observed, indeed, that the illusion exists not only when different sizes with the same weight are compared, but that it also remains when the weight of the larger size is sensibly higher than the one of the little size.

This observation allows to establish the approximate limit of the magnitude of the illusion.

It was thus possible for us to work in two different ways:

(1) In a first group of subjects the first comparison was made between B. 500 and b. 500. The subject had only to say which of both blocks seemed to him to be the heavier. If there was an hesitation, he was allowed to lift both blocks again. After this first test, the comparison was to be made between B. 550 and b. 500; then, between B. 600 and b. 500 and so on, till the comparative lifting of B. 1100 and b. 500.

(2) For a second group of subjects, the order was the same, but the first comparison was made between B. 1100 and b. 500, the second between B. 1050 and b. 500, and so on, till the pair B. 500—b. 500.

We think that the magnitude of the illusion, as we have intended to establish it, may be rated in a sufficiently objective way by the physically heaviest weight for which the illusion is still extant. Under these conditions, we have an illusion, not only if the subject perceives the heavier weight of a pair as the lighter, but also if he has the impression that the two blocks, which are to be compared, are equally heavy.

EXAMPLES

I

B 500 < b 500
B 550 < b 500
B 600 < b 500
B 650 < b 500
B 700 < b 500
B 750 = b 500 (limit of the illusion)
B 800 > b 500
B 850 > b 500
B 900 > b 500
B 950 > b 500
B 1000 > b 500
B 1050 > b 500
B 1100 > b 500

II

B 500 < b 500
B 550 < b 500
B 600 < b 500
B 650 < b 500 (limit of the illusion)
B 700 > b 500
B 750 > b 500
B 800 > b 500
B 850 > b 500
B 900 > b 500
B 950 > b 500
B 1000 > b 500
B 1050 > b 500
B 1100 > b 500

III

B 500 < b 500
B 550 < b 500
B 600 < b 500
B 650 < b 500
B 700 > b 500
B 750 < b 500 (limit of the illusion)
B 800 > b 500
B 850 > b 500
B 900 > b 500
B 950 > b 500
B 1000 > b 500
B 1050 > b 500
B 1100 > b 500

IV

B 1100 > b 500
B 1050 > b 500
B 1000 > b 500
B 950 > b 500
B 900 > b 500
B 850 > b 500
B 800 < b 500 (limit of the illusion)
B 750 < b 500
B 700 < b 500
B 650 < b 500
B 600 < b 500
B 550 < b 500
B 500 < b 500

V

B 1100 > b 500
B 1050 > b 500
B 1000 > b 500
B 950 > b 500
B 900 > b 500
B 850 < b 500 (limit of the illusion)
B 800 > b 500
B 750 < b 500
B 700 < b 500
B 650 < b 500
B 600 < b 500
B 550 < b 500
B 500 < b 500

VI

B 1100 > b 500
B 1050 > b 500
B 1000 > b 500
B 950 > b 500
B 900 > b 500
B 850 = b 500 (limit of the illusion)
B 800 < b 500
B 750 < b 500
B 700 < b 500
B 650 < b 500
B 600 < b 500
B 550 < b 500
B 500 < b 500

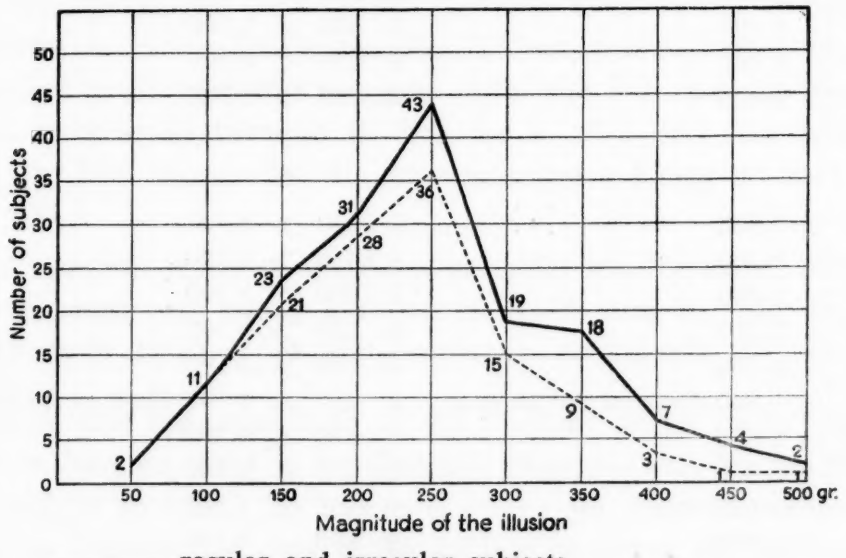
The great majority of subjects manages in a regular way (examples I, II, IV and VI) to establish the limit of the illusion. Others manage to do it less regularly or irregularly (examples III and V).

When we consider the method which we have applied to establish the magnitude of the illusion, we cannot help thinking of the possibility of an influence of each lifting upon the following ones, influence which might consist, for instance, of a certain perseveration or a certain automatism in the appreciation of the weights. In order to examine the eventual interference of such a factor, we have established the following conditions:

We shall see further on that the modal and median magnitude of the illusion obtained by our means is 250 gr. in the ascending proof. Consequently, we have asked 100 different subjects to compare right away B. 750 with b. 500. 71 subjects have found the little block heavier and 7 found both weights equal. 78 % of these subjects have thus shown the illusion in the isolated lifting B. 750—b. 500.

These same 100 subjects have immediately, after the comparison B. 750—b. 500, been submitted to the ascending proof: in this case, just as in the usual ascending experiment, a less great number of subjects (45 %) reach or overreach the mean magnitude of the illusion, i.e. 250 gr. This observation is opposed to the idea of the efficient interference of a factor of automatism in the determination of the magnitude of the illusion by the process of successive comparisons.

Subjects: For the whole of our researches about the magnitude of the



——— regular and irregular subjects.

---------- regular subjects alone.

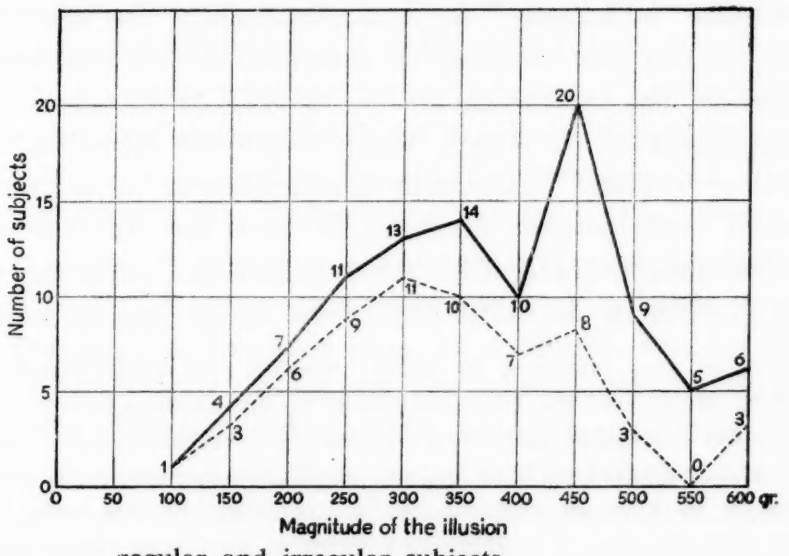
Fig. II

illusion we have contacted 260 normal subjects. They belonged to all kinds of social and professional standings. Their age varied between 20 and 60 years; both sexes were present in approximately the same number.

Results of the measurements of the magnitude of the illusion:

A. The scale of the progressively increasing weights (from 500 to 1100 gr.) has been applied to 160 subjects:
   (1) 127 subjects, i.e. 79.3 %, have reached the limit of the illusion in a regular way;
   (2) the magnitude of the illusion in all these subjects, the regular ones as well as the irregular ones, varies between 50 and 500 gr.;
   (3) in the regular subjects, the most frequent magnitude of the illusion is to the amount of 250 gr. (fig. II).

B. The scale of progressively decreasing weights (from 1100 to 500 gr.) has been applied to 100 subjects:
   Here follow the results:
   (1) 61 % of the subjects have reached the limit of the illusion in a regular way.
   (2) Among the 100 regular and irregular subjects, 6 show an illusion right from the beginning (an illusion of 600 gr.); the lower limit is of 100 gr.
   (3) In the regular subjects, the most frequent illusion-magnitude is of 300 gr. (fig. III).



———— regular and irregular subjects.

---------- regular subjects alone.

Fig. III

If we compare the results of the ascending method with the ones of the descending method, we are struck by some differences:

(1) The illusions are generally greater through application of the second method:

(a) if we take into consideration the results of the regular subjects only, the modal frequency is of 300 gr. against 250 with the first method;

(b) if we take together the results of the irregular and the ones of the regular subjects, the most frequent illusion is of 450 gr. with the second experience, whereas it remains of 250 gr. in the first experiment;

(c) with the application of the second method, 6 subjects show the illusion right away, i.e. in the comparison B. 1100—b. 500, whereas, with the application of the first method, no illusion at all was so high.

The difference of the magnitude of the illusion observed in the results obtained with the second method, when compared with the one observed in the first method, may be explained by the fact of an "anticipation". It is known that such a fact of anticipation is practically constant when the subject has to find back, in a series of ascending weights, the one which he had formerly lifted. Under these conditions, the subject "anticipates" by under-estimating the previously lifted weight. In our two methods of determination of the magnitude of the illusion, the subject has in fact to find, in an ascending or in a descending series, the block whose weight he estimates is equal to the one of b. 500. Consequently it is justified to think that there too, an act of "anticipation" interferes. If this is the case, the subject will under-estimate the weight he has to find in the ascending series, and will over-estimate the same one in the descending series.

It has been our intention to verify whether the under-estimation in the ascending series and the over-estimation in the descending series of weights do show for the blocks used for our researches. To this end we have examined 60 subjects, who had all been ordered to lift the median block (800 gr.), then to trace it back in an ascending series consisting of our large blocks from 500 to 1100 gr., and in a decreasing series of our large blocks from 1100 to 500 gr. Between the ascending and the descending proofs, the subject had to lift again the weight of 500 gr. In order to eliminate as much as possible the eventual influence of one lifting upon the following, we have begun with

30 subjects, with the ascending proof, and with 30 others with the descending proof. We have thus observed that the order in which these proofs were done exerted no influence upon the results of this research. The research has taught us that all our subjects had a strong trend to anticipate as much in the ascending as in the descending proof. In the ascending proof, indeed, 58 subjects have shown a mean anticipation of 153 gr. (median: 150 gr.); 1 subject has found the actual weight; another one has transgressed largely the weight he was to trace back. In the descending proof, 51 subjects have shown a mean anticipation of 157 gr. (median: 150 gr.); 5 subjects have traced back the weight of 800 gr. and 4 others have chosen a lower weight. Thus, the majority of these 60 subjects has anticipated in both ways; all these subjects have anticipated at least in one way.

It goes without saying that this experiment of the "anticipation" departs rather sensibly from our experimental conditions of the research of the illusion-magnitude. This experiment, however, pleads in favour of the interference of the factor "anticipation". If there is a reason to admit the intervention of this factor, it explains why the limit of the illusion-magnitude is sensibly higher in the descending experiments than in the ascending ones.

(2) The irregular subjects show a clear tendency to be so in the zone of the high illusions. They are furthermore more numerous in the descending experiment. This should not astonish us, for the subject whose illusion-limit is not situated in the left half of our curves (fig. II and III), reach a zone of the scale in which the distinction of the weights becomes more delicate according to the law of Weber-Fechner and the conclusions of the researches of Harper and Stevens about the subjective appreciation of weights. We have wished, however, to verify whether the appreciation of differences of 50 gr. would really be more difficult for the heaviest weights of both our scales. To this end, the large blocks have been grouped in pairs whose elements differed only to the amount of 50 gr. The six pairs resulting from this way of grouping have been presented successively to the subject, with the order to indicate the heavier block in every pair. This presentation has been done in a definite, but not progressive, order: 550—600, 1050—1100, 750—800, 950—1000, 650—700, 850—900. The lighter block of the pair had, in each case, to be lifted first. Each subject had to make three successive liftings for each pair, and had, consequently, to give three appreciations for each pair. In this way, the 100 subjects of this experiment have supplied 1800 comparative liftings.

Three responses were possible: *a*) correct appreciation of the difference

of the weights; *b*) impression of equality of the two blocks; *c*) inversion of the correct answer.

We have grouped the responses in two different ways:

(1) on the one side, the responses upon the liftings of the three inferior pairs and, on the other side, those upon the liftings of the three superior pairs;

(2) on the one side, the responses upon the liftings of the lightest pair and, on the other side, those corresponding to the heaviest pair.

TABLE II

Couples	W > w	W = w	W < w	Erroneous answers
3 light couples	772	89	39	128
3 heavy couples	720	136	45	181
lightest couple	277	20	3	23
heaviest couple	214	62	24	86

According to the results gathered in table 2, one finds a sensibly higher number of erroneous responses for the heaviest weights. This teaches us that the appreciation of the differences of weights is more difficult in the superior half of our scales.

6. Examination of the Eventual Influence of some Factors upon the Frequency and the Magnitude of the Size-Weight Illusion

a. *Frequency*

Because of the great rarity of the absence or the inversion of the illusion in normal subjects (0,83% of all our cases under the different experimental conditions), it is not possible to examine, according to the number of our cases, the influence of eventual factors upon the frequency of the illusion.

b. *Magnitude*

We were in a condition to examine the eventual influence of two factors: (1) sex, (2) educational level.

(1) *Sex*: We have computed the limit of the illusion in 50 regular male subjects and 50 regular female subjects in the ascending experiment. We have found a mean limit of 726 grams in the males, and of 742 grams in the females. In the descending experiment, upon 43 regular male subjects and 43 regular female subjects, we have obtained a mean limit of 806 grams with the males and of 815 grams with the females.

The feeble difference between these mean data in the males on the one side and the females on the other side, puts us in a position to conclude that the magnitude of the illusion is practically the same for both sexes.

(2) *Educational level*: because of the great number of our subjects, all considered as normal, it was not possible for us to establish their psychometric level by a sufficiently differentiated battery such as the scale of Wechsler or a factorial scale of the Thurstones. We are of opinion, however, that the development of the capacities of intellectual efficiency is for a great part implied by the educational development of our subjects. Therefore, in order to make the comparison as significant as possible, we have constituted two extreme groups: on the one hand the university-trained subjects, on the other hand the unskilled labourers.

In the ascending experiment, the mean limit of the illusion has been established upon two groups, consisting each of 32 regular subjects. A mean limit of 726 grams was found in the university-trained subjects, and of 750 grams in the unskilled labourers.

In the descending experiment, we have computed the means of the illusion-limit in 40 regular subjects: 20 universitary ones and 20 unskilled labourers. In the first group, we have found a mean limit of 825 grams; in the second, a limit of 797 gr.

These differences are again too little significant to allow us to conclude with an influence of the educational level upon the magnitude of the illusion.

Other factors, such as the age or the profession, could not be examined, because of the very unequal numerical representation of these factors among our subjects.

7. Attempts at an Explanation of the Phenomenon of the Size-Weight Illusion

The explanations of the size-weight illusion are varied, since the first observations made by Charpentier. This author ascribed the phenomenon to a difference in the tactile sensations: the largest object, coming in contact with a larger skin-surface, would have its weight spread over a greater number of sensorial receptors, and consequently would seem lighter.

Flournoy (1895) proved in a simple and convincing way the error of Charpentier, when the latter ascribed the illusion to the intervention of the "sensations of innervation": if the objects which are to be compared are suspended to rings into which one slips one's finger, the illusion remains, although the tactile differences can no longer interfere. The sight alone gives us, under these conditions, the appreciation of the size and is sufficient to create the illusion, which disappears when one closes one's eyes.

Several authors have looked for the explanation of the size-weight illusion in a surprise-phenomenon, following a preperceptive disposition of the subject. So G. E. Müller (1889) mentions a "getäuschte Erwartung" which he theoretically analyses in the following way: "When we are in presence of two objects of a different size, we assume that the larger is also the heavier. Because of this expectation, the motor-impulsion of the subject's arm will be too strong to lift the larger object, and this latter will be lifted too easily. On the opposite side, there is an inverse disposition when the smaller object is lifted: because of too feeble an impulsion it will be lifted with difficulty. These data will cause the larger object to seem lighter than the smaller one."

Flournoy adopts a similar point of view and considers that the subjects feel the illusion, because they lift with greater swiftness the object which their judgment considers a priori as the heavier (1894).

This explanation however does not seem very satisfactory. One may give it a value of some importance when it concerns a first lifting, but, as was already pointed out by Seashore, the weight-illusion is an extremely tenacious phenomenon, which is not corrected, even when the subject is made perfectly aware of the experimental situation. How is it then possible to explain, under these conditions, the keeping up of the illusion only by a "getäuschte Erwartung"?

Notwithstanding the judicious character of the remark of Seashore and of other authors, we have wished to verify experimentally up to which

point we have to retain the interference of a feeling of surprise. We have asked 10 university-trained subjects to lift our weights B. 500 and b. 500 with the order to tell which of both was the heavier. After this, we have asked them to compare the weights again, 4 times successively, insisting before each lifting upon the necessity of adapting their effort in function of the weight-difference perceived in the course of the first lifting. In none of these subjects, the illusion has disappeared or has been inversed, but, in seven among them, the difference seemed to be less; for the three other subjects, the little block became more and more heavy. A similar experiment has been done by asking 10 other university-trained subjects to compare a metal-weight of 500 gr. with B. 500. Before the first lifting they were told that iron is a heavier substance than wood, but that this, however, did not imply that the iron weight was necessarily heavier than the wooden block, as the latter was very much larger. They all have found the metal-weight heavier. After the first lifting, we have asked each of these subjects to make four new successive liftings, each time adapting their effort to the result of the first comparison. During these series of liftings, 9 of the subjects have maintained the illusion till the 5th. experiment. 5 of the subjects have found that the difference between the two weights was decreasing, but that the metal-weight, however, remained the heavier. 4 subjects have had the impression that the metal-weight seemed to be even heavier than in the course of the first comparative lifting. 1 of the 10 subjects has given irregular responses: with the second lifting, he has found that the difference between the two weights was diminishing; with the third, that the wooden block had become heavier; with the fourth, that the iron weight seemed heavier again than the wooden one, and with the fifth, that both weights seemed equal.

From these complementary researches, it appears that the surprise may play a certain part, but that it is not sensible to look upon it as the explanation of the illusion.

The difference of size between both weights is perceived in a tactile (cutaneous and kinesthetic) and visual way. We may wonder whether each of both modes of perception has the same importance in the phenomenon of the illusion here studied.

According to the quantitative researches of A. Ley, the tactile perception would be prevalent.

We were of opinion that it was not without interest to verify this problem by using our material.

We have applied our ascending method upon 60 new subjects. A first

group of 30 subjects was blindfolded, whereas, for the second group of 30 subjects, the liftings were made with free sight, whilst the blocks were lifted by means of a strap by which the weight was hung at the palmar side of the top of the index-finger. In the experiment without visual percep-

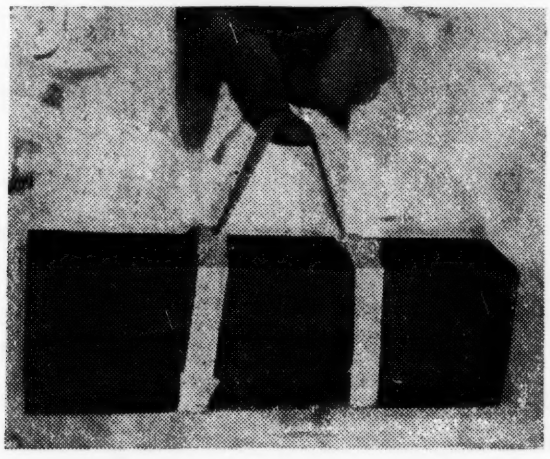


Fig. IV. Exclusion of the tactile and kinesthetic sensibility.

tion, we have obtained a mean magnitude-limit of illusion of 263 grams (modal and median values: 250 gr.). Let us here remind of the fact that, when sight and tactile sensations intervene simultaneously, the modal and median magnitudes of the illusion were also to the amount of 250 gr. On the other hand, as far as this experiment is concerned, when the sight alone was operating, the mean magnitude of the illusion was only of 181 gr. (modal and median values: 150 gr.).

So it would well seem that the tactile and kinesthetic sensibility intervenes for a greater part in the production of the illusion.

According to Van Biervliet, the illusion would not depend on a rating of the weight, but from an appreciation of the ratio weight/size, i.e. of the density. He based his hypothesis upon the following consideration: if a subject has to lift a bottle by holding it by its neck, it will appear heavier to him than if he grasps it by the bottom. Now, it concerns the same object, whose weight has not changed. In the first situation, however, the volume looks smaller than in the second, which modifies the ratio w/s and causes the object to look heavier. It has been objected to the explanation given by Van Biervliet that density is an abstract notion, and that we have here a mathematical ratio which is quite ignored by the majority of the individuals.

It should be remarked however that the perception of heaviness as an

immediate phenomenal datum corresponds to this notion of density. So, even the uneducated man, who ignores all about the physical notion of density, may very well be able to perceive degrees of density. Through maturation and experience, he becomes able to get aware of the phenomenal propriety "heaviness" which is entirely different from the objective propriety "weight".

A similar explanation has already been given by P. Koseleff (1936 and 1946), when he makes a distinction between "phenomenological heaviness" and "physical property of weight", and by I. Huang (1945) when he writes: "In everyday life, we have the impression of an object as light or heavy *for its size*. A certain range of densities, in other words, seems to have come, probably empirically, to be accepted as normal for objects in general".

Summary

In their researches upon the frequency and the magnitude of the size-weight illusion, the authors have used a scale of 13 wooden parallelepipeds of the same size, but of a weight varying from 500 till 1100 gr., showing a difference of 50 gr. per block. Each of these large blocks had to be compared with a little one of a weight of 500 gr.

The experiments have been made upon a total number of 650 normal adults.

The size-weight illusion was present in 99,17% of the cases.

The illusion exists not only when we have two equal weights of different sizes, but also when the weight of the larger size is markedly superior. This fact makes possible to measure the magnitude of the illusion for the maximum difference of weight with which the illusion persists.

The comparison has been made in two different ways: in a first group of subjects the little block had to be compared with the large ones in an ascending order of the weights; in a second group, the order was descending.

For the first group, the most frequent illusion was to the amount of 250 gr.; for the second, it was of 300 gr.

A complementary research allows to assume that the well-known fact of "anticipation" in the tracing back of a weight in a series intervenes in the difference of the illusion-magnitudes in both fore-mentioned methods.

Neither sex, nor the educational level exert an influence upon the magnitude of the illusion.

The different explanations which have been given are taken into consideration, are criticized, and some are experimentally studied. The most acceptable interpretation is the one of an acquired capacity to perceive, in an immediate phenomenal datum, the heaviness (density) of the lifted object.

Résumé

Dans leurs recherches sur la fréquence et la grandeur de l'illusion poids-volume, les auteurs ont utilisé une échelle de 13 parallélépipèdes de bois de même volume

mais d'un poids variant de 500 à 1100 gr, à raison d'une différence de 50 gr par bloc. Chacun des grands blocs devait être comparé à un bloc plus petit pesant 500 gr.

Les expériences ont été effectuées sur un ensemble de 650 adultes normaux.

L'illusion poids-volume était présente dans 99,17 % des cas. L'illusion existe non seulement quand il s'agit de deux poids égaux de volume différent, mais encore quand le poids du grand volume est nettement supérieur. Ce fait permet de mesurer la grandeur de l'illusion par la différence maximale de poids pour laquelle l'illusion existe encore.

La comparaison a été faite de deux façons différentes: chez un premier groupe de sujets on a fait comparer le petit bloc avec les grands suivant un ordre ascendant des poids; dans un second groupe l'ordre a été descendant.

Dans le premier groupe, l'illusion la plus fréquente est de 250 gr, dans le second elle est de 300 gr.

Une recherche complémentaire permet d'envisager que l'anticipation, habituelle lors du repérage d'un poids dans une série, intervient dans la différence de la grandeur de l'illusion dans les deux procédés précités.

Ni le sexe, ni le niveau éducationnel n'influencent la grandeur de l'illusion.

Les différentes explications possibles de l'illusion sont passées en revue, critiquées et étudiées expérimentalement.

L'interprétation la plus vraisemblable est celle de la capacité acquise de percevoir en une donnée phénoménale immédiate la « lourdeur » (densité) de l'objet soupesé.

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THE EVIDENCE FOR THE PERMANENTLY DISTURBING EFFECTS OF MOTHER CHILD SEPARATION

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INTRODUCTION

Two recent books by Bowlby (7, 8) have aroused considerable interest because in them he has advanced the point of view that children deprived of their mother's care or that of a permanent mother substitute during their first two years of life, are likely to suffer intellectually, physically or emotionally from the deprivation. These two books further suggest that the effects of the deprivation are permanent. The author contends that the view advanced is supported by strong evidence.

Any interest in the conditions governing the healthy mental and physical development of children must command our attention as these books have done. The contention of this article is that in a subject of such outstanding interest a careful analysis of evidence is essential and that although Bowlby presents evidence, his case is by no means conclusive.

Thus, whatever may be thought of the conclusion reached by Bowlby, it is considered on the basis of a re-examination of the evidence he presents (7) that the hypothesis that children separated from their mothers suffer permanent damage must be considered as still a hypothesis. Part of the reason for this state of affairs is undoubtedly the fact that a clearly stated dynamic hypothesis is not presented. Theory to explain the supposed effects of deprivation is either scanty or lacking and is nowhere systematically examined. Lack of a clear testable proposition is a considerable shortcoming of the work, but a further confusing feature of the method of presentation in the book is that canons of evidence adopted to establish the thesis, are not the same as those used to judge the weight of objections against it. In addition, relevant evidence is overlooked.

It is necessary to examine the argument advanced by Bowlby and the

evidence adduced in its favour. The scheme of discussion is as follows. Firstly, a statement of Bowlby's is quoted from his monograph (7), and the lack of a clearly stated theory criticised. Evidence which aims to show the effects of deprivation is then examined section by section, dealing separately with retarded speech, growth retardation, failure of emotional response and intellectual backwardness. The argument against Bowlby's hypothesis is then stated and examined.[1]

The argument advanced in "Maternal Care and Mental Health".

The best way to present Bowlby's argument is in his own words. "For the moment it is sufficient to say that what is believed to be essential for mental health is that the infant and young child should experience a warm, intimate, and continuous relationship with his mother (or permanent mother-substitute) in which both find satisfaction and enjoyment. Given this relationship, the emotions of anxiety and guilt, which in excess characterise mental ill-health, will develop in a moderate and organised way. When this happens, the child's characteristic and contradictory demands, on the one hand for unlimited love from his parents, and on the other for revenge upon them when he feels that they do not love him enough, will likewise remain of moderate strength and become amenable to the control of his gradually developing personality. It is this complex, rich, and rewarding relationship with the mother in the early years, varied in countless ways by relations with the father and with siblings, that child psychiatrists and many others now believe to underlie the development of character and of mental health .... Thus, a child is deprived even though living at home if his mother (or permanent mother-substitute) is unable to give him the loving care small children need." Various forms of deprivation exist but Bowlby regards complete separation from the mother as the worst and institutional care as the common form of this complete deprivation. He states that "Complete deprivation, with which we shall be dealing principally in this report, has even more far-reaching effects on character development and may entirely cripple the capacity to make relationships." The effects referred to are "deprivation leading to anxiety, excessive need for love, powerful feelings of revenge, and, arising from these last, guilt and depression". Bowlby states that the parent-child relationship is the foundation of the subsequent mental health or ill-health

[1] This treatment of the material presented by Bowlby is not meant to be an elaboration of his own experimental work only. It aims to consider arguments presented by a number of investigators whose work has been most comprehensively summarised in Bowlby's monograph.

of the children. He writes that his intention in the book is to concern himself with the maternal relationship only, and avers this to be the most important. P. 13 "It is she who feeds and cleans him, keeps him warm, and comforts him. It is to his mother that he turns when in distress." After this brief résumé, and an acknowledgment of the difference between maternal separation theories and constitutional theories of mental disturbance, Bowlby refers to the importance of embryology as providing a model for separation theories and then states the evidence for the effects of deprivation.

A brief summary of the alleged effects is as follows: backwardness in talking (p. 16), drop in IQ (p. 19), poor locomotor balance (p. 20), retarded physical growth (p. 28), shallowness of emotional response (p. 31), aggressiveness and distractibility (p. 39), impairment of the capacity for abstract thinking (p. 55) and inadequate capacity as a parent and neglect of children (p. 154–155). The dynamic theory which is held to explain the defects listed is dealt with primarily in Chapter 5 of Bowlby's monograph with supplementary material in other pages.

The theory of maternal deprivation

There seem to be two separate and distinct theories which are offered in different parts of the monograph. The first states that the formation of ego and superego through continuous relation with one person uninterrupted by separation is important for the formation of character. The second offered in other parts of the book lays stress on the importance of a stimulating environment in the development of abilities and character. For example, this is clearly stated in relation to poor conditions (7, p. 5) as follows. "In these conditions, the child has no opportunity of learning and practising functions which are as basic to living as walking and talking." Both these theories are concerned to explain what is essential to the development of an emotionally, physically and mentally healthy person, but further light is thrown on the first theory by Bowlby's explanation of the traumatic effect of separation in terms of the toddler's limited sense of time. According to the theory, the small child is unable to estimate the length of time that a separation will involve. It is suggested therefore that he regards the separation as permanent and furthermore regards it as a punishment.

It may be unfair to ask for a clearer and more precise theory of the dynamics of maternal deprivation, but it is none the less necessary to have such a theory if we are to attempt to plan the educational or developmental programme for the pre-school child. It becomes necessary to know, for

example, whether maternal deprivation or lack of a stimulating environment is relatively more disturbing. Bowlby appears to be also aware of the distinction.

The theoretical issues involved, though important, are not so indissolubly bound to the clinical and experimental evidence that each of the points may not be dealt with, in their turn, without necessarily drawing theoretical inferences. The first point to be considered in such evidence is that for vocal and verbal backwardness.

Backwardness in Verbalisation

Bowlby quotes the work of Brodbeck and Irwin (9), which showed that in orphanage children those under six months are "considerably less vocal than those in families, the difference being clearly discernible before two months of age." (p. 16). Burlingham and Freud's studies (11) are also quoted to establish the deleterious effect of deprivation on vocal development. Burlingham and Freud's material is explained by them in terms of love deprivation but Brodbeck and Irwin (9) hold, as does Goldfarb, that vocalisation and use of words among young children depends on opportunity for imitating adults: if the main person to be imitated is constantly changing the child's problem of learning will be increased. Their theory is similar to the social learning and imitation hypothesis of Miller and Dollard (40), and they draw attention to the apparent differences of opinion between Burlingham and Freud on the one hand and Ribble (48) on the other. Whereas Burlingham and Freud (11) assume that maternal deprivation leads to vocal backwardness and withdrawal, Ribble, (48), expects it to lead to overcompensatory verbalisation.

This confusion, think Brodbeck and Irwin, results from not adopting the social imitation theories. At the same time they point out that some orphanage children achieve levels of verbal competence which are superior to those achieved by children living with their parents in middle class homes characterised by a high verbal level. This Brodbeck and Irwin explain on their own hypothesis by suggesting that some children will be selected for particular attention by a nursery nurse, or will have a particularly long period of contact with her and hence find learning particularly stimulating.

One is obliged to conclude that so far as theory is concerned the crucial experiment remains to be done and that although in the greater proportion of cases children in residential nurseries tend to be inferior in verbal development under present conditions, the exceptions show that this need not be the case.

As we are dealing at present with the material offered by Bowlby, we can turn to the second matter dealt with, that of retarded physical growth.

Growth Retardation

The evidence for a connection between physical growth and maternal deprivation is much more tentative. The case as reported rests on three pieces of research. One of these examined in detail (20), shows the following: it may be inferred from case histories that emotional disturbances lead to growth failures; secondly, that after transfer to institutions children, whose growth is already retarded, presumably due to emotional causes, may improve. The evidence for the first of these statements is not convincing. However, the second contention appears to be well supported. For example, after six months in a particular institution 15 to 22 per cent of children tend to show growth failure, whereas at admission the percentage will be between 75 and 90. Thus, despite the presumed adverse and irremediable effects of emotional upset, about 53 per cent as a minimum estimate improve. These are all children living in institutions. Unfortunately, of the four cases quoted all were over six years on admission, so that no evidence covering the period asserted by Bowlby to be critical is available.

Finally, it may be said that institutions can provide the conditions for emotional adjustment resulting in return to normal physique in a high percentage of cases, whatever may have caused decline prior to admission. Many of the findings concerning physical effects of institution life are based on the kinds of institution which are no longer the rule in England, and there are clearly some cases where institutions are better than some homes.

Bowlby says of growth experiments: "These results, if confirmed, are clearly of the greatest interest, opening up new possibilities of research into the interrelation of psyche and soma and providing the clinician with a simple and reliable tool." This statement is cautious, but even so, not cautious enough. The growth of many of the children improves considerably according to these figures. The permanently adverse effect of separation does not appear to be established in this study.

Failure in Emotional Response

This supposed effect of deprivation has been emphasised by a number of authors, and perhaps most of all by Goldfarb (27, 28), working in America.

As Goldfarb seems to have done most of the work in this field, his articles can be dealt with as representative. But theoretically Goldfarb is not in quite the same position as Bowlby, although he is quoted extensively

by him (7). Thus, after referring to his work in Chapter 3, Bowlby says: "For skilful planning and care of execution, Goldfarb's work ranks high; not until a comparable piece of work has been done with different results can there be reason to doubt his findings." (p. 39). One of Goldfarb's conclusions about home life as opposed to institution life (23), is stated as follows: "It is probable that no matter how well both kinds of care are organised, there will be some groups of children most effectively reared in one kind of atmosphere as against another." He also points out, in the same (23) article that children from foster homes as compared with children from institutions suffer more from certain emotional problems and he mentions "passively anxious timidity, shyness". He states further (p. 259) in talking of children in foster homes, "On the other hand, the foster home children seem to show more problems involving withdrawal behaviour and anxieties growing out of confused family relationships. In general, it would seem that the institution children, when they develop problem behaviour, quite consistently show an attacking aggressive trend. The foster home children also evidence some of the same problem behaviour but show a more distinct trend toward what has been described as the passive or "escape" pattern of problem behaviour." Although the institution children in this particular study were more disturbed than foster home children, the evidence consisted of checklisting behaviour items such for example as "disobedient", "apparent craving for affection", "inability to concentrate" and "complain easily" which however meaningful clinically are notoriously subjective and unreliable unless rigorously checked and counterchecked by observers who do not know of the hypothesis.

Intellectual Backwardness

Goldfarb's difference of viewpoint from Bowlby's is also made clear when he distinguishes between rejected children living at home and institution children. Whereas Bowlby is sometimes indecisive about distinguishing the two groups, Goldfarb clearly states his point of view. The distinction made by Goldfarb is evidently based on Hullian reinforcement and association concepts of learning whereas Bowlby's epistemology is less clearly defined and he sometimes regards rejected children as similarly deprived to separated children and sometimes as deprived in a different sense. Goldfarb always speaks of the former as suffering from emotional conflicts but not necessarily from unstimulating environment and of the latter as suffering from unstimulating environments, but not necessarily from emotional conflicts. (25).

Intellectual backwardness is the area in which most experimental

evidence exists for maternal deprivation and it will be instructive to consider some of Goldfarb's studies, on which Bowlby relies so largely. In one study Goldfarb (25) compares two sets of children matched for age and sex, one group having been in a foster home from the age of 4 months to 34 months and the other in an institution for the same period. The mean IQs for each group were 68 and 96 for the institution and foster home groups respectively at the age of 34 months. At this point the institution group transferred to foster homes and at the age of 43 months IQs were tested again and IQs of 75 and 101 obtained for the institution and foster home groups. Thus, although both had changed, their relative positions had remained unchanged. The inference drawn by Goldfarb is that the institution period had damaged the children intellectually to such a degree that nine months of foster home care was unable to repair the damage or to begin to do so. But there was no adequate matching of intelligence at four months, nor could there be at this age and it is therefore possible that the difference in IQ noted at 34 months was also present at 4 months. A more strictly controlled experiment would be needed to justify the inference which Goldfarb makes.

Another study of Goldfarb's draws the same pessimistic conclusion. He retrospectively examines (27) the early history of two extreme groups, one rated as adjusted and the other as extremely maladjusted. Ratings were carried out by Social Workers on the basis of school, home and community case records. Differences between the adolescent groups were held to be due to length of time in institutions in early years (25 months as opposed to 34 months) and to age at institutionalisation. Differences were significant at the 5 per cent level and may be regarded as reasonably satisfactory providing only that we accept ratings of behaviour at second hand as a basis for judgment, a procedure condemned by Bowlby when used by Brown (10), but condoned in Goldfarb's work. It is necessary to take account of the findings even though Bowlby, in dealing with discrepant evidence tends to reject similar material, such as the personality inventories, used by Brown (10).

Goldfarb has concluded (28) "... the institution children present a history of aggressive distractible uncontrolled behaviour. Normal patterns of anxiety and self-inhibition are not developed. Human identifications are limited, and relationships are weak and easily broken. There are retarded social competence and a definite trend to inferior intellectual achievement." In view of the wide differences in IQ at both stages of the investigation it is difficult to be sure that deprivation was the main cause of the failure of the institution group to improve, and the reliance in judgment of emotional

behaviour on subjective ratings of imprecise definition, must both be held to count as objections to such a conclusion.

In these studies of intellectual retardation Bowlby does not mention the investigations with orphans carried out by the Iowa school (53, 54, 58, 59), although these are widely known. It cannot therefore be held that his evidence, even if it were more conclusive, would be very strong without attempting some explanation of the discrepant findings of these workers. He does take account of some contrary evidence however, and the extent to which he does this is now reported.

Contrary evidence noted by Bowlby

Bowlby mentions some papers which he finds unsatisfactory, those of Orgel (44), Brown (10) and Bodman (6). His objection to Orgel's work is that it is based on 16 children and is unsystematic. However, much the same comments could be made on the work of Fried and Mayer which Bowlby accepts. Brown's work is criticised for failing to use objective techniques of observation. Goldfarb's work may be criticised on this basis and such an objection can therefore be sustained against both investigators. Brown seems to have failed to note the ages at which his institution group went to an institution; he found no difference in neurotic tendency between a home reared group from bad homes and an institution group.

The most recent work noted by Bowlby in which views contrary to his own are to be found, is that of Bodman (6). Bodman quotes Goldfarb and Freud and Burlingham and so is aware of some of the evidence used by Bowlby. He points out that their findings are in contradiction with one another. Bodman also points out that in Bowlby's work, 9 of 11 cases who showed the supposed effects of maternal deprivation, had a history of violent alcoholic or anti-social parental behaviour. Bodman's own work compared 50 institution raised children with 52 senior school children living at home. They were tested and interviewed in their last school year and then tested and interviewed and their employers interviewed six months to a year later. The Social Maturity Quotient on the Vineland Scale showed a difference of 13 points in favour of the children living at home.[2] However, when the institution sample was differentiated into those with abnormal and those with normal heredity, the normal institution group with normal heredity was found to show the same score as the children with normal heredity living at home.

[2] The Vineland Scale tends to be biassed in favour of children living at home, who may, for example, have had access to telephones and hence know how to use them, etc.

Bodman then reports his analysis of Lowrey's original cases and finds 16 fully reported of whom two thirds have parents with inadequate, unstable or psychotic histories. He also makes the point about Goldfarb's IQ study, which has been dealt with above, namely that Goldfarb's institution children may have been originally mentally defective in view of their low IQ of 68. Bodman calls in question the scientific propriety of not attempting to control parental history carefully and in addition shows that age of institutionalisation need not be relevant to social development.

Bowlby objects in turn that there are no tests of significance in Bodman's work and says that the effects of each variable should have been isolated by holding others constant. Presumably he means that the effect of heredity should have been held constant and the influence of environment or experience judged in this way. One way of doing this would be by matching institution and home children for bad heredity and good heredity, and comparing the difference in social adjustment between the two groups at the two levels. This is what Bodman did. Another objection of Bowlby's is that the children brought up at home had been separated from their parents during the war for various periods, and that some of the institution children had not gone to the institution until after the age of four years, that is, after what he considers the crucial period. This objection is on a par with Bodman's objection to Bowlby's data that a high proportion of his cases had a bad heredity. Both must be regarded as objections which prevent the drawing of a firm conclusion from the evidence presented, and for this reason one is forced to regard both conclusions as unestablished.

Contrary Evidence Not Considered by Bowlby

The two main sections of information not considered by Bowlby have been summarised by Orlansky (45) on the one hand, and by Skeels and his colleagues at Iowa on the other.

This evidence can be considered conveniently under these two divisions. Orlansky, concerning himself with emotional development, makes a sectional consideration of the possible influences of early training. He deals with nursing experiences, mothering, sphincter training, restraint of motion and infant frustration and aggression. Under these headings is considered much evidence provided by clinical and anthropological workers. Under "nursing experiences" Orlansky considered such problems as breastfeeding versus bottle feeding, and demand versus schedule feeding. No one of these issues is taken up explicitly by Bowlby, although all may be involved in his overall hypothesis. The same is true of sphincter training, restraint of motion and frustration and aggression. Mothering, however, may be

presumed to be at the core of the deprivation hypothesis. Whether or not mothering means something more than the items of behaviour listed above remains to be seen, but it is dealt with independently here.

Orlansky offers extensive evidence to show that the effects of early conditioning or training may be overlaid or changed as a result of later, for example childhood, experience. If this is the case, then the effects of early deprivation may be neither so great, nor yet so permanent as Bowlby's thesis demands. Orlansky's theory appears to imply that in a society which expected bad results from separation of mother and child these might occur, not because of rupture of the relationship between mother and child but because of the treatment accorded to the children after separation.

Orlansky considers that the prepotent influence of the first two years of life upon subsequent behaviour has been over-valued. He states: "If it can be shown that major structuring of personality takes place after the first year or year and a half, clearly the contribution of infant experience in general and of these disciplines in particular to subsequent personality formation has been exaggerated by some therapists. We believe that this is the case, and will call attention to some of the evidence that has led us to this conclusion. The picture, however, is not entirely clear, and the honest investigator must confess that apparently discordant facts are faced at every stage of the inquiry. These facts must be reconciled, and not ignored, before any solution of the problem can receive general acceptance" (p. 28).

Ethnological data adduced by Orlansky do not support the view that character becomes fixed in early life. Evidence of this kind can be extended and one of the chief points which can be made is that later events may confirm or compensate an early impression or behaviour form. Thus Murphy (42, p. 657) says, "a satisfying infancy does not necessarily compensate for economic deprivation in the next ten years, and there is increasing evidence that later gratification may go far toward offsetting the effects of early frustration." Orlansky adds, "We conclude that the rigidity of character structuring during the first year or two years of life has been exaggerated by many authorities, and that the events of childhood and later years are of great importance in reinforcing or changing the character structure tentatively formed during infancy." (p. 28, 42). He considers that there is a tendency for psychiatrists and psychologists to review the behaviour of infants in terms of their own motives and that such an approach is unsophisticated.

The main theoretical issue raised by such a discussion is that concerning

the possibility that certain kinds of learning take place at only one precisely defined stage of development and no other. This phenomenon has been demonstrated in animals by Tinbergen (56), by whom it has been called imprinting. The evidence for imprinting in man is not so strong however. Most impressive perhaps is that presented by Hebb (32) from the records of Senden (52) and Riesen (49). But this has been ably criticised by Wertheimer (60), and Hebb himself seems to take up the position that the question is undecided. So he says (p. 119) "New learning is facilitated by old. But . . . it may be limited or canalized as well." Furthermore, evidence exists in the contrary sense that early learning may not facilitate later learning as we see from the maturational studies with twins carried out by Gesell and Ilg (22); and from the work of Bingham and Griffiths (4) it may be inferred that early educational handicap is not necessarily a barrier to complete recovery later. Furthermore, considerable confusion continues in this field in so far as an implicit identification of "imprinting" and the phenomenon reported by Senden is sometimes made. Such an identification is not justified on present evidence.

The supposition that critical stages exist for humans as for animals, would appear not to be established on the evidence available. The most that can be said is that in certain limited and specific cases early learning will facilitate later learning, but lack of it will not necessarily prove a complete bar to acquiring knowledge later.

The present state of affairs is authoritatively summed up in a very complete review of the literature on early experience and later behaviour in animals, by Beach and Jaynes (3). After dealing with imprinting, learning, emotion and temperament among other subjects, they state, "Reviewing the studies in the foregoing pages has left the authors with two basic convictions. The first is that a fuller understanding of the effects of early experience upon subsequent behaviour is fundamentally important for a science of psychology and for the broader field of animal biology. A second and equally strong impression is that much if not most of the presently available evidence bearing upon this problem is equivocal and of undetermined reliability."

The Iowa Studies

Part of Bowlby's hypothesis is that deprivation leads to backwardness and that such backwardness is permanent. That backwardness associated with deprivation need not be permanent is shown in the work carried out by Skeels and his colleagues (53, 54, 58, 59) at Iowa. The essence of this work is that children who, whilst living in an orphanage or institution are

daily given some academic training, show ultimate levels of literacy and competence in routine arithmetic either equivalent to or in excess of that of the normal children of the homes or the social class from which they come.

Perhaps the most representative study is that of Skeels, Updegraff, Wellman and Williams (54). The mean age of the children in this study varied according to the group into which they were divided, but was either more than 35.9 months or less than 44.6 months. The range of ages covered was from 18 months to 5 years six months. Groups were matched by pairs and treated identically except that one group was given a pre-school curriculum denied the children in the other group. There is no exact information in the monograph concerning the ages at which the children entered the orphanage in which the study was carried out, but the fact that some resident at the beginning were only 18 months old indicates that at least some fall within the period regarded as critical by Bowlby. In addition, some of the language used in describing the children is reminiscent of similar language used by Goldfarb and Bowlby. For example: (from p. 23 to p. 24 of the monograph), "The attitude toward adults was a strange mixture of defiance, wish for affection, and desire for attention. It was rather startling to find that there was little desire for the teachers' approval; the children seemed to crave attention but whether that attention was due to disapproval or approval mattered little. There seemed to exist what might be termed a feeling of the individual against the world, expecting no quarter and giving none..." Much more about lack of constructive play, reaction against authority and aggressiveness could be quoted in the same sense.

After residence under experimental or control conditions of up to 400 days or more, IQ on Binet scales showed differences in changes on originally matched mean IQs of 9 points, 16 points for those above 80 IQ and for those of IQ 50 to 79, about 5 points. These figures have been criticised by Quinn McNemar (38) on a number of counts, chiefly failure to consider regression, rapport variation from experimental to control group and failure to take account of differences of significance of an IQ from the same test for different ages.[3] A reply by Wellman, Skeels and Skodak (59) in the same journal shortly afterwards deals with some of

[3] I am indebted to Dr. H. J. Eysenck for pointing out that an objection can be made against the Goldfarb and other studies comparing the intelligence of children at infancy and adolescence. This objection is that tests of infant intelligence measure locomotor ability and co-ordination, and tests at adolescence verbal or abstract ability, and the two are therefore not comparable.

the points raised and in a short note following this reply McNemar failed to repeat his criticisms and he left this study out of statistically faulty researches which he listed. It is impossible to re-examine the whole issue here but it is fair to say that some changes consistent with hypothesis took place in this study and that later work has tended to confirm the original findings. Of those given pre-school training and above 80 IQ, 35 gained more than 5 IQ points compared with 4 of the control group. Forty seven changed up or down 4 points compared with 12 of the controls, and 18 only dropped more than 5 points compared with 85 of the controls. Changes among those of lower than 80 IQ were in the same direction though not so impressive. It might therefore be said that under some circumstances, life in an institution can be stimulating and improving.

Social competence as measured by the Vineland Social Maturity Scale was much improved in those boys given experimental training, as compared with the controls, although subsequent losses were noted in both groups and the picture is not as clear as might be wished. Most important are the observations on behaviour. Pre-school children showed more willingness to accept and make friendly advances over a period of about 6 months. Unfortunately no comparison was made with control children's behaviour but it is important in view of Goldfarb's experiments with transfers to foster homes, to know that improvements in the disturbed who are also deprived, can occur. In the same Iowa study series, Page (46) was able to show that the orphanage children given training in self confidence showed increases in this trait among those originally not well endowed. There is thus some evidence that children separated from their mothers can undergo modifications of behaviour as well as of intelligence. Skeels and his colleagues were also able to show that motor skills could be improved among orphans when training was given and that other untrained orphans failed to improve to the same extent. The difference would be regarded as insignificant by present criteria, being significant at about the 7% level, but in the right direction. The same scale of performance, the McCaskill-Wellman (37) test of motor achievement was used in this study as in another by Goldfarb (26) in which transfer to foster homes proved ineffective in improving institution children.

Other examples of improvement among the backward could be listed, which show that pessimism concerning their level of achievement is not always justified, but they do not bear directly on the hypothesis of maternal deprivation. One study might be referred to, however, in which separation from the parents resulted in an overall improvement in performance on an intelligence test. This is a study by Clarke and Clarke (14). Working in a

mental deficiency colony, the authors examined the IQ's of recent admissions from homes where there had been gross neglect and cruelty. They also examined the admission IQ's of other certified residents. Re-examination after about 2 years showed large changes in the first group and relatively small changes in the second. The difference in mean gain is significant at the 1% level, those from very bad homes gaining on an average ten points of IQ on the Wechsler Scale, as compared with a gain of four points on average by other admissions. Gains occur in the neglected group during their stay in hospital. It is necessary to add that, despite the merits of English hospitals for mental defectives, they are not perfect, and an even better environment might have been found for such patients. Nevertheless, this environment produced the results quoted. It can be argued that the bad home conditions had been responsible for a temporary depression of the realisable IQ of the people concerned.

It can be seen from this evidence that Bowlby's case for the permanently disturbing effects of maternal deprivation is matched by a case which states the contrary thesis, that later learning can improve the separated children, and that early lack of opportunity to acquire knowledge is not always a barrier to acquiring it later, even though the child has been separated from its mother.

The Administrative implications

Administrative policy must take account of both inadequate homes and inadequate institutions. Pre-school training in Day Nurseries for example may be more adequate and stimulating than the environment of some homes. So little is known of the best developmental conditions for young children that many homes now regarded as adequate, faute de mieux, may be found to be unsuitable. There is some danger that too great stress on the dangers of mother child separation may divert interest from the development of research into the optimal cultural conditions for infant intellectual and emotional growth. No scientific curriculum of pre-school training has yet been projected, much less worked out in detail.

Thus, whilst adequate physical standards exist in residential nursery schools, we are not yet in a position to lay down standards of any kind to guide the education and early training of infants. The opposition to the institutional housing of children, when left as a vague and sometimes emotional opposition to institutions is virtually also an opposition to situations in which a satisfactory development routine can be worked out in practice.

When attention is turned to the residential school, it is sometimes argued

that better schemes of care, such as Scattered Homes, should be instituted. However, the study of the home is in need of attention as much as the institution. In some institutions relatively stable staff membership and genuine affection for the children are operative conditions. In some homes inadequate discipline, lack of affection, or oversolicitude is the rule. Many studies, such as those of Radke (47) and Bishop (5), show this to be the case.

Unless the problem is viewed with due attention to evidence, some areas of influence may be over-emphasised and others neglected. It seems likely, according to some of the evidence given above, that social pressures at later ages have effects on people which may modify the predisposition created by earlier training. There is certainly strong sociological evidence that the current state of mental health in England is to some degree a function of social conditions. The high rate of neurosis, the geographical distribution of mental deficiency and psychosis are facts which need explanations in terms of multiple social and genetic causes. There is some danger that by analysing one source of emotional disturbance, such as mother-child separation, the interaction of the society and the family may be neglected, and the family considered as if it were an isolated unit, which alone determines the behaviour of individuals in all their social relations. Current sociological research suggests that this is not the case.

Ferguson and Cunnison (21) found that cases of delinquency seldom included deprived children. Perhaps it is in the organisation of institutions, not in their form, that the chief danger lies, a viewpoint consistent with Miller and Dollard's hypothesis and Goldfarb's use of it. At least some piecemeal consideration of the problem seems desirable.

There is no intention in this discussion of advocating the institution as a form of care when good family care is available. In fact its disadvantages in its old form are well known. It is argued, however, that changes should be made as far as possible on the basis of sound theory and sound evidence. In this case caution may preserve for the children committed to the care of a local authority some of the advantages inherent in the family of our day, whilst avoiding the disadvantages and incorporating some of the good points of community life.

Summary and Conclusions

The results of this survey tend to undermine confidence in the hypothesis of maternal deprivation and resulting social, intellectual and physical inadequacy, and it is certain that part of the reason for this is the imprecise way in which the

hypothesis is set out. It never emerges clearly whether or not the results of deprivation are due to some unstated lack of mothering, or to the unstimulating nature of institution environment as it is to-day. If the latter is meant, then the short term remedy may be to improve our institutions. Goldfarb's theoretical position is based on such a learning theory and does not support a "mothering" hypothesis. Therefore Bowlby's own statements and those of Goldfarb are not identical.

In summarising the literature in favour of the deprivation hypothesis Bowlby appears to have been lenient to investigators who support his view, but, in criticising a limited number of opponents, has applied criteria of a degree of strictness which, when applied to the investigations which he thinks support his case, would destroy it. He has failed to take account of the contrary evidence surveyed by Orlansky and that contained in the work of the Iowa Child Development Centre. Later evidence, which he could not know of when his monograph was published, shows that investigators still get results which contradict his claims concerning the permanence of early handicap.

The evidence quoted in his monograph is fairly presented, but conclusions arrived at by one author may conflict with those of another. Thus Brodbeck and Irwin (9) disagree with Burlingham and Freud (11) and with Ribble (48), and Goldfarb (24) with Bowlby (7). In a like manner it is not precisely true to say that Brodbeck and Irwin (9) or Irwin (35) have shown that deprivation invariably leads to backwardness in speech and vocalisation; a deprived child's performance can be better than the average for a mothered child from a stimulating home. Bowlby presumes that Goldfarb and Irwin both agree with him when in fact they adopt a theoretical point of view distinct from the one which he adopts. Here however the picture is not defined because Bowlby never states in detail how deprivation operates. Evidence quoted proves that institutions are in certain instances beneficial.

Theoretical problems which Bowlby takes for granted as solved, are still in question. Perhaps the most important of these is the hypothesis that one can reason from animals to humans about learning and imprinting. There is enough evidence to demonstrate imprinting in animals, but with man evidence is lacking. Therefore it may not be assumed that later learning is always prevented by early impressions or the lack of them, and Bowlby's pessimism may not therefore be justified.

On the basis of such theoretical objections and on the basis of the evidence either offered or ignored, it must be said that Bowlby's thesis is not established. Whilst suggestive and valuable in drawing attention to the neglect of children which sometimes occurs, its lack of exact logic in theory and the conflicting evidence on which it is based prevent this author from accepting the theory as it stands.

In conclusion it should be said that here is a need to work out the first steps of a psychology of parenthood. There is some danger that the later misdemeanours of children and indeed much that is psychologically wrong with children will currently be attributed to bad upbringing, that is to parents.

A member of the American Psychological Association, writing in the American Psychologist for December, 1953, makes an analysis of this blind spot in psychology. She points out that parents have never been recognised officially as a branch of psychology by the A.P.A., that on the other hand child psychology has been intensively cultivated, that what research exists in relation to parents is concerned with their "goodness" or "badness" vis a vis children, that the rejecting mother has received undue attention and that the literature emphasises too much the rights of

infants and insufficiently the rights of parents. She concludes that too much research is of the "mother blaming" kind. "A study of the mother-blaming complex itself", she concludes, "should comprise a part of the training of psychologists . . . Awareness of the source, the nature, and the consequences of this tendency may help the . . . psychologists assume a more mature attitude in choice of research problem, in content of teaching, and particularly in clinical practice."

Bowlby's book has drawn attention to a very important problem of wide consequences and interest. For this reason it is vitally important that its claims and evidence should be tested and evaluated in as detached a manner as possible.

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CONSCIOUSNESS AND MEANING

BY

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I. Introduction

Professor Humphrey has pointed out (2) that there is no "problem of meaning" as long as the situation where meaning arises is not taken out of the general context of behaviour. Despite these cautionary remarks, discussions on the psychology of meaning are frequently met. In this paper, an attempt is made to describe the circumstances under which something is said to become meaningful. The problem is seen as a special case of "unconscious processes". The theme developed here seems implicit in the writing of many contemporary psychologists.

II. Thinking

Most psychological hypotheses about conscious thinking can be tested only on oneself; but, with any luck, supporting evidence may come from what other people say about their own thinking.

Such evidence supports the following hypotheses:

(a) the stream of thought is determined through association and set (the latter being compounded from the goal-direction—*Aufgabe*—and from the preceding conditions of the environment and the organism); (b) sometimes the content of thought has no obvious connection with what the content was a moment before (insight, imageless thought, incubation); (c) sometimes trying too hard may not be a good thing in problem solving; (d) thinking seems to "arise" when action is "frustrated". This last (d) suggests that thinking might be considered as a substitute for behaviour, and it is reassuring that (a), (b) and (c) hold for overt behaviour as well as thinking, provided the right translations are made. For instance, "association" in (a) might be seen as a parallel for conditioning, response generalization and stimulus generalization. The above hypotheses refer to the *dynamics* of thinking. The *content* of thought, on the other hand, refers to such entities as ideas, images, and, perhaps, feelings. If the

dynamics of thinking are to be explained in terms of its content, recourse must be made to talking about unconscious thinking. In particular, the Aufgabe, the sudden discontinuities in streams of thought, and the phenomenon of incubation are all explained in terms of unconscious processes.

This is unsatisfactory for two important reasons: (a) unconscious thoughts are assumed to be like conscious thoughts, and of this we can have only indirect evidence, even in ourselves; (b) there is no good reason why mental events should be expected to show continuity, i.e. should be explicable in terms of causal relations. Continuity might be expected to hold for the total organism plus its environment, but a great deal of activity within the nervous system is never represented in consciousness, and many of the *nuances* of consciousness cannot be represented in terms of words, images, etc., i.e. the content of thought.

The problem would be solved to some extent if thinking were to be explained in terms of observable behaviour rather than behaviour explained in terms of the content of thought. From this approach, a psychological investigation of thinking would fall into two parts: (a) to give a behaviourist account of behaviour (paradoxically, not ignoring *clues* to be obtained from introspection, but being prepared for "red herrings"); (b) to ask questions of the kind: which aspects of behaviour are reflected in consciousness? under what circumstances are these aspects reflected in consciousness? do they ever cease to be reflected in consciousness? The first part of this programme is the concern of most experimental psychologists. Here we shall consider prematurely the three questions under (b).

III. Consciousness

Regarding the first two questions, it is often said that consciousness arises when a novel adaptation has to be made. This is an incomplete answer, since one may be highly conscious of the sensations from a warm, relaxed bath, when presumably one is not facing any urgent problems of adaptation. There is a sense, however, in which it can be said that consciousness becomes *canalised* during problem solving situations which involve some delay in response. (d) above says this in a different way. During such states, stimuli not relevant to the problem solving will usually not be reflected in consciousness.

However, in this paper, interest is to be centred on the third question: under what circumstances do aspects of behaviour cease to be reflected in consciousness? The following evidence suggests that one such circumstance is the occurrence of *repetition*. For instance, if a stimulus is repeatedly

presented (e.g. the tick of a clock, the pressure of clothing) it will cease to give rise to sensation; habituation is said to have occurred. (The most noticeable exceptions are noxious stimuli, and stimuli which recur sufficiently rapidly to cause summation). During the acquisition of a skill, processes which have been repeated again and again during the early stages of acquisition will cease to be reflected in consciousness. They are then known as lower order habits. For instance, in learning to use a typewriter, the learner may first concentrate on making connections between the letters and their appropriate keys, but later this "letter-habit" will cease to be reflected in consciousness. It is interesting that, once a skill has been acquired, it is often still possible to "bring back into consciousness" a lower order habit, but when this happens the skill tends to disintegrate. In general, a habit which has been repeated sufficiently often will cease to reach consciousness if some higher order (more complex? more unusual?) adaptation is being made. Similarly, when learning a new language, in the initial stages the student will consciously search for the right associations. For instance, given the word "dog", he will consciously search (perhaps with the aid of mnemonics) for the word "chien". When this performance has been repeated several times "dog" will automatically lead to "chien", the intervening links having ceased to reach consciousness. (It might be argued that those links no longer exist; and the same argument might apply to many instances where unconscious processes are postulated; i.e. instances where a person behaves *as if* there had been an intervening process which had not been reflected in consciousness. However, if the person be encouraged to analyse the situation, he may well *relearn* the original associations, and the missing links will reappear, as though they had been unconscious all the time).

Some of the most exciting examples of this tendency-to-become-automatic come from the field of perception. When a student is first introduced to geometry, he will consciously work out relationships within figures; if this occurs sufficiently often, the relationships will be immediately presented when he looks at similar figures; they will be perceptually "given", his perception will have become structured. Workers in the field of personality-and-perception have accumulated many examples of individual differences in perception which show that learning results in information being highly selected and structured before it reaches consciousness. It seems more than likely that some of this selecting and structuring was at one time conscious, but has ceased to be, through repetition. The equivalent rooms of Ames are dramatic examples of such structuring of information before it reaches consciousness. This suggests a reappraisal of the phenom-

enological approach. For instance, the fact that a Subject is "immediately aware" of an organisation in the perceptual field should not be used as a criterion that the organisation is innately determined. Perceptual habits which have been conscious may, through repetition, have ceased to be reflected in consciousness, but the Subject may behave as if they were still operating.

An analogy may help in the exposition. Being conscious is like observing a newspaper editor who has started from small beginnings. When quite young, he did his own reporting, edited the copy, set up the type, printed and sold copies of his own paper. As he became more affluent, he was able to train reporters, type-setters, and so on, to use his own methods. He now sits in his editorial office. The news which passes through his hands has been collected by reporters, highly preselected by his sub-editors, and later passed on to the public by other of his minions; but their methods of working will bear the stamp of his early experience and training. Normally he will be concerned only with policy considerations for the paper, and most of the selecting and despatching will be carried on with a minimum of interference on his part. Some branches of the organisation may be almost autonomous (Classified Ads.?). But if the editor wants to, he can leave his office and visit one of his sub-editors, or go out and do his own reporting again. If he does so, there is the possibility that the organisation may break down.

IV. Meaning

As a child grows, many stimuli, processes, movements, may be reflected in consciousness, but if they are repeated sufficiently often, they may cease to be so reflected. (There are exceptions. Skinner's work (5) on variable-ratio reinforcement suggests, by analogy, that repetition may fail to have this function if the outcome of the repeated activity has been unpredictable). It is possible, for instance, that a young child is conscious of visceral sensations which in adults are normally never conscious. Similarly, the child will make connections between objects and words, but these connections, if once conscious, may cease to be so with repetition. When this occurs the word is said to have acquired meaning. The situation is akin to that of the student learning a new language. He will pass through the stages of consciously connecting words; the connections will cease to reach consciousness, but there will still be a process of translation; finally he will eventually be able to "think" in the new language. Similarly, the child will eventually be able to think in his new language, which will be his native tongue. The switchboard experiment of van der Veldt (6) shows a

similar progression. Subjects had to learn to make arm movements in response to combinations of nonsense syllables. First the learnt connections, and then the "schemata" standing for the words, ceased to reach consciousness, and the arm movements themselves were said to have acquired meaning. Here is an instance of a response which is not verbal acquiring meaning. In the course of everyday experience, many such automatic, non-verbal, "meaningful" responses may be acquired in this way. In the case of such responses being visceral, it might be expected that introspection would yield reports such as "there is an unanalysable feeling-tone of meaning", or "I feel as though I knew what it meant, but I cannot verbalise it" or "there is an image of meaning".

V. Other viewpoints

Messer, in his classification of Bewusstseinslagen (3), made some important comments on the awareness of meaning. He suggests three categories of Bewusstseinslagen: (a) where the meaning (of an object, word, etc.) is immediately presented and reportable; (b) where there is an unanalysable Bewusstseinslage of meaning; (c) where a word is accompanied by an image of meaning. Most examples of category (c), for instance the one quoted by Humphrey, could be equally well classed under (a), since the associations to the stimulus were immediately and consciously presented. According to the thesis developed in the present paper, category (a) would be typified by the person who immediately knows the meaning of "chien", that is, who could immediately say "dog", or draw or point to a dog. Category (b) would include the cases where the immediate associations are non-verbal, and may be not expressible in verbal terms.

In section III (Consciousness), it was suggested that intervening links might not only cease to reach consciousness, but they might actually cease to exist. This viewpoint was put very precisely by Höffding (1). He states: "Not only may conscious results come from unconscious working up, but there may be also unconscious intermediate links in the midst of conscious work. Supposing the idea *a* to be linked with he idea *b*, and *b* again with *c*, then *a* will finally produce *c* directly, without the intervention of *b*. A proposition, which we have learned to understand by way of proof, remains in our consciousness after we have forgotten the proof. All education rests on the possibility of the intermediate links thus sinking below 'the threshold' ".

Many contemporary writers use the word "meaning" in a way which implies agreement with the theme developed here. For instance, if a light and an electric shock are repeatedly paired together, it is frequently said

that the light becomes a sign for the shock, or that the light *means* shock. Osgood (4) has considered the problem in detail. He considers that a sign refers to or means a particular object because it gives rise to a mediation process which includes some portion of the behaviour towards the object itself. This differs from the present description which states that meaning does not arise unless intervening links between sign and response, or partial response, cease to reach consciousness, or, perhaps, cease to exist. This seems more in line with common usage of the word meaning, and more in line with the way it was treated by introspectionists.

VI. Summary

It has been argued that, in thinking, some aspects of behaviour are "reflected in consciousness"; that these aspects may cease to reach consciousness through repetition. When this happens in the case of an object's being linked with a verbal response, the response acquires "meaning" when the intervening links cease to reach consciousness, or, perhaps, cease to exist. This description is briefly compared with some other viewpoints.

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AN EXPERIMENTAL STUDY ON THE INFLUENCE OF THE MENTAL SET ON THE PERCEPTION OF IDENTITY AND SUBSTITUTION

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BY

J. EX AND G. L. DE BRUIJN

1. INTRODUCTION

It is a fact of common observation that one and the same objective entity can be perceived in different ways. Thus the same line-square with crossing diagonals can be seen either as "a square with a cross within" or as "four linked triangles". It is on this possibility that puzzle pictures are based, and Gestalt-psychologists have devised numerous configurations allowing of an ambiguous structuration, such as "the faces-bowl", "the rabbit-duck figure", "the old and the young woman" and "the reversible cube".

We know that, in spite of the great changes in the retina stimuli, an object is perceived as identical with itself. The cat passing behind the fence continues to be one and the same cat, notwithstanding its having disappeared from our field of view for some time. An object undergoing a change will keep its identity and will be experienced merely as "changing", at any rate under certain conditions.

A good many investigators in the field of sensory perception have stated that the so-called "mental set" plays a role in the perception of certain phenomena. (Some terms for "mental set" found in German publications are: "Aufmerksamkeitsverteilung", "Beobachtungsrichtung", "Auffassungsweise", "Einstellung".)

Most investigators, however, have not yet gone into a systematic study of the influence of this subjective factor on certain phenomena of perception.

It has been the aim of the present study to investigate this role of the mental set on the perception of identity and substitution.

2. NATURE OF THE PROBLEM

The tachistoscopic substitution of a circle by a triangle will not give an impression of identity. A faintly curved line, however, when substituted by a straight line of the same length, will seem to straighten itself out.

If more complicated figures are used, the conditions to which the figures have to answer in order to continue to give that impression of identity, will become more complex too.

Now the problem investigated in the present study is:

Of what nature is the influence of the mental set on the perception of identity between a configuration S (starting-figure) and a configuration E (end-figure) on tachistoscopic substitution of S by E?

By "mental set" is meant here the attitude of the individual with regard to a non-varying objective entity in so far as his attitude is decisive for the presence of different phenomenal structures.

In carrying out the experiments use was made of line-figures. For the S-figures the patterns chosen were such that they could easily be seen in more than one way. The E-figures, however, have only a single form; all of them are letters and appear, when shown separately, directly and spontaneously in their letter meanings (for the S-E combinations see Table IV). It proved quite impossible to see each of these E-figures otherwise than as a letter, whatever shift of the mental set was attempted.

The S-figures were constructed with the E-figures for a basis, by changing the position of letter elements.

Before each observation S and E are superposed in such a way that in the tachistoscopic shift from S to E the identical *elements* of the figures do not change their positions; only the non-identical elements will appear in a different place.

Example:

S E Superposition

3. Arrangement

Observations were made by four subjects (O^{1-4}), O^1 being a trained observer, O^{2-4} untrained.

a) With O^1 a variant of the Michotte tachistoscope *) was used. (Fig. 1)

O looks through an objective at two laterally placed card holders containing cards with figures, which figures by means of an optical device are seen in the same visual field. By pushing a button, O can put the tachistoscope into action, i.e. make two sliding screens cover one of the instrument's two windows and expose the other.

*) Michotte, A., Description et fonctionnement d'un nouveau Tachistoscope de comparaison, *Arch. Psychol.*, XII, No. 45, 1912.

The screens DD slide along the vertical rod G. They are fitted at different heights and are moved rapidly upwards by means of a spring. Lighting is regulated through an electrical resistor: both cards, when viewed through the tachistoscope, can be made to be equally illuminated. The figures (see

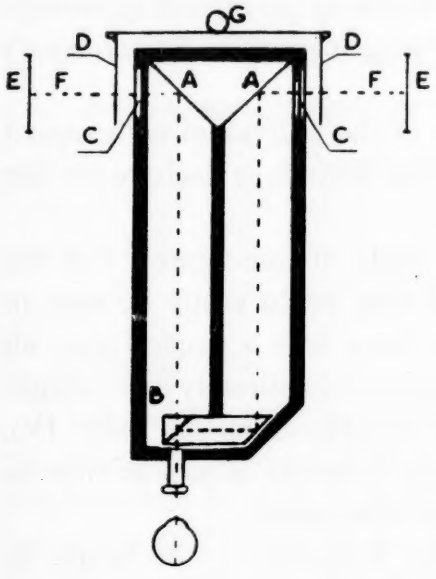


Fig. 1.

A = mirrors
B = prism with semi-transparent side
C = window
D = screens
E = card holders
F = ray of light
G = vertical rod

Table III) are drawn in Indian ink on cards measuring 10 × 10 cm; they have a height of 2 cm; thickness of lines 0.3 mm; distance between E's and observer ± 58 cm.

b) With O^{2-4} a different apparatus was used because the arrangement as described sub a) proved to be less suitable for untrained observers. Observation by means of apparatus *b* turned out to be easier and less fatiguing to the subject; there was also the additional advantage that this type of arrangement allowed the experimenter to follow easily the descriptions of the structures perceived by the subject.

The figures were alternately projected in red on a moderately lighted

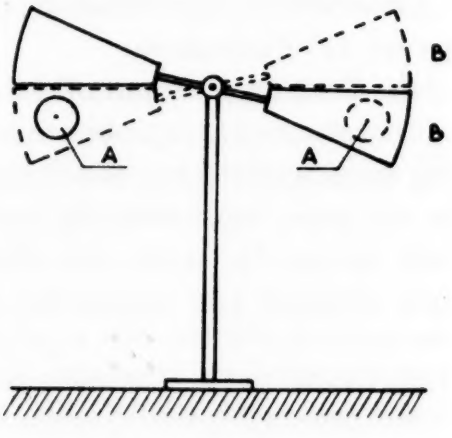


Fig. 2

A: projector B: "propellor"

Shifting-time S-E < 0.1 sec.;
Length of projected figures: c. 20 cm.;
Thickness of lines: c. 3 mm.;
Distance O — screen: 5 m.

screen by means of two projectors. The apparatus is worked by pulling a string; a catch is released and a string causes a "propellor" to turn, its blade freeing the light of one projector and screening the other. (Fig. 2)

4. PROCEDURE

To all subjects nine S-E pairs were presented in the ways as described above.

O^1: O was asked to have a good look at S, to give a verbal description of it, and to put the tachistoscope into action. Next he was asked to report his impressions of the S-E shift. Each S-E pair was presented twice in succession. Arrangement as sub 3a.

O^{2-4}: As O^1, but with the 3b arrangement, each pair being presented three times.

As O^{2-4} were rather naive individuals and their descriptions both of the S-figures and of the S-E shift often turned out to be too vague to determine with certainty whether they had received an impression of identity or not, in handling the results (see Table IV) only those answers were marked *s* (substitution) or *i* (identity) when the relevant descriptions offered a high degree of probability in this respect. The other answers were marked *?*, indicating that it was not beyond doubt whether O had observed identity or substitution.

The same prudence was exercised in classifying the descriptions as: "S seen as a surface-figure" and: "S seen as derived from E-figure" (see Table IV).

5. RESULTS

a) If S is seen as a surface-figure, this view will favour, in shifting from S to E, an impression of substitution.

b) If S is seen as a letter-derivate, this will tend, in shifting from S to E, to produce an impression of identity.

All of the letters have the character of a line-figure. The letter-derivates (S-figures), however, can be seen:

1. as letter-derivates;
2. as figures having a surface-character;
3. otherwise.

Examples (of 1 and 2):

S E

sub 1: If I see S as a letter W with a line on top of it, I view this figure as a letter-derivate. When shifting from S to E, I will either get an impression of a growing letter W, the horizontal line disappearing, or I will see that line "opening up". Thus I will see one same letter W presenting a change. In this case the term *identity* is used.

sub 2: S may also be viewed, however, as two tops side by side. In that case the white spaces within the lines will not appear as "empty" to my view, but as the surfaces of the tops. On observing the S-E shift I will get an impression of the tops disappearing and a letter appearing instead. In this case the term *substitution* is used.

sub a): In handling the data obtained through the observations made by the four subjects the following procedure was adopted:

We collected all descriptions from which it was evident that O had seen S as a surface-figure. Next, these descriptions were checked for O's impressions of the S-E shift and arranged according to *i* (identity), *s* (substitution) and *?* (impression indefinable). The outcome was that 58 % of the observations proved to be cases of substitution; not a single case of identity occurred (0 %); in 42 % of the observations the phenomenal shift had been described in such a way as to make it impossible for us to infer with adequate certainty whether substitution or identity had been observed or not.

Table I incorporates the results of those observations in which O saw S as a surface-figure.

TABLE I

The starting-figure seen as a surface-figure

In S-E Shift	O^1	O^2	O^3	O^4	Total	%
Impression of substitution....	5	7	3	7	22	58 %
Indefinable impression	1	3	5	7	16	42 %
Impression of identity	0	0	0	0	0	0 %
Total.....	6	10	8	14	38	100 %

(Detailed results of the investigation will be found in Table IV).

sub b): The second step in handling these results was to determine the number of cases in which S had been seen as a letter-derivate. In such a case O would describe S as "a letter-plus-(or minus)-something".

Examples:

S-figure 1.

described as "a W with arms";

S-figure 2.
described as "an H with slanting legs".

Next, each of these letter-derivate cases was checked for O having observed substitution or identity in the S-E shift.

The outcome of this check was that 87.5 % of the observations in which the S-figures had been seen as letter-derivates, were cases of identity; there was not a single substitution (0 %); in 12.5 % of the cases no certainty could be obtained whether O had observed substitution or identity (see Table II).

TABLE II

S seen as a letter-derivate						
In S-E Shift	O^1	O^2	O^3	O^4	Total	%
Impression of substitution....	0	0	0	0	0	0%
Indefinable impression	0	0	1	1	2	12,5%
Impression of identity	3	5	3	3	14	87,5%
Total.....	3	5	4	4	16	100, %

c) In the course of the investigation we were struck by another phenomenon, viz. the fact that repeated presentation of one same pair of figures is apt to produce an impression of identity. More and more O got an impression of S being one same changing thing (identity), less and less did he see S disappearing and E appearing. Only O^1 reported the latter phenomenon.

Its absence in the other subjects is most probably due to a slight change in the instruction, the subjects being asked at a second presentation of the same pair of figures to start from a different phenomenal structure of S. Thus they were asked to see a figure , when presented for the second time, as "a W with a line on top of it", and at the third time as "two tops", at least if they experienced no difficulty in seeing these structures and keeping to them. O^1, however, at the second presentation of the S-E pair, did not receive any suggestions with regard to the initial structure.

Table III records O^1's impressions of two successive presentations of one same pair of figures.

(Note: The total number of S-E pairs presented to O^1 was 35, the data of which are tabulated below. To O^{2-4} were presented 9 S-E pairs each. Of these 9 pairs 7 were among O^1's 35 pairs. Those 7 pairs of O^1 are also incorporated in Table IV.)

TABLE III

Impressions of tachistoscopic S-E shift received by O[1]

1st trial	2nd trial	Number of times this combination occurred
s (substitution)	s	6
i (identity)	i	8
? (indefinable)	?	9
s.....	i	5
i.....	s	0
s.....	?	3
?.....	s	0
?.....	?	0
i.....	i	4

These data, if not very numerous, point to a rule well-known and easily verified in daily life: *Expectation structures that which is given, in a direction parallel to what is expected, thus favouring an impression of identity.*

It may be stated here, by way of hypothesis, that this structuration-force of expectation makes itself the more felt as the expectation with regard to its structure is the more pregnant and unambiguous, the extant phenomenon, on the contrary, the more amorph and unstable.

d) *Detailed results of the investigation* (see Table IV on page 205).

6. CONCLUSION

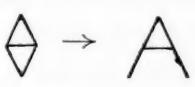
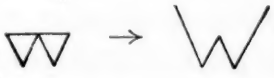
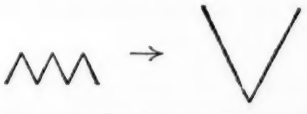
The legitimate conclusion from the results given above would seem to be:

If one same set of stimuli allows of different phenomenal structures, the phenomena observed in the tachistoscopic shift from S(tarting)-figure to E(nd)-figure—the latter allowing of only a single possible structuration—will be co-determined by the observed initial structure of the S-figure.

Consequently the integration of two phenomena into one dynamic process of identity is dependent on the degree of similarity between the phenomenal nature of both phenomena, not on the two sets of stimuli underlying those phenomena.

It is only with very simple and phenomenologically unambiguous sets of stimuli that the influence of the mental set can be disregarded, e.g. when working with pairs of squares or circles. In similar cases the influence on the

TABLE IV

Pairs of S-E figures	O	Impressions of S-E shift			S seen as surface figure			S seen as derivate of end-figure		
		1 *)	2	3	1	2	3	1	2	3
1	1	?	?							
	2	s	i	s						
	3	?	?	s						
	4	?	?	?						
2 	1									
	2	s	?	?	×					
	3	?	?	s	×					
	4	?	s	?	×	×				
3	1									
	2	s	i	s	×		×		×	
	3	?	?	?						
	4	?	s	s	×	×	×			
4 	1	s	i		×				×	
	2	s	i	s			×		×	
	3	s	?	?			×		×	
	4	s	i	s	×		×		×	
5	1	s	i		×				×	
	2	s	i	s	×		×		×	
	3	?	i	?	×		×		×	
	4	?	?	?	×		×		×	
6	1	?	i		×				×	
	2	s	?	?	×					
	3	s	s	?						
	4	?	?	i						×
7	1	?	?							
	2	s	i	s					×	
	3	i	i	?				×	×	
	4	s	i	?	×				×	
8	1	s	?		×					
	2	?	?	?	×		×			
	3	s	s	s			×			
	4	?	?	?			×			
9 	1	s	s		×	×				
	2	?	?	?	×					
	3	s	?	s	×	×	×			
	4	?	s	?	×	×	×			

*) 1, 2, 3: 1st, 2nd, 3rd presentation.

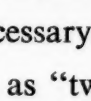
process of identity exercised by such factors as differences in seize and color can be exactly investigated. But whenever the initial set allows of various figural interpretations it will need careful examination which structure existed at the moment of shifting to the end-figure.

When conducting such investigations in actual practice it would seem a matter of importance to choose the stimuliset of the end-figure in such a way as to permit of only one well-defined structuration.

The problem which gave rise to the experimental investigation as reported on above, was:

Of what nature is the influence of the mental set on the perception of identity and substitution, on objective, tachistoscopic substitution of S by E?

Now the present investigation has proved that even with simple figures it is not possible to determine adequately the stimuli-sets necessary for a process of phenomenal identity. It was also shown that with the same sets of stimuli both identity and substitution could be perceived. Consequently perception did not depend on the stimuli-sets but on something else. It was the so-called mental set that conditioned which of the two impressions would be realized. By this is meant that, if S allowed of various phenomenal structurations and E of a single one only, a perception of identity was experienced in the S-E shift if the "direction" of the phenomenal S-data corresponded with the phenomenal E-data.

Thus in order to perceive a relation of identity between S and E  it is necessary that S should be presented phenomenally as "a-W-plus-line", not as "two tops".

In the latter case the S-E shift is perceived as a *discontinuity:* S disappears and E appears. If, however, S is phenomenally present as "a-W-plus-line", the S-E shift is mostly perceived as *a continuity.*

SUMMARY

If a subject is shown a simple line-figure and this figure is substituted tachistoscopally by a second one, the impression upon the subject of the shift from one figure to the other will be influenced by the subject's mental set. Either he may get the impression that one figure disappears and another figure appears (substitution) or he may perceive the first figure changing (identity). Whether in these experiments substitution or identity was perceived appeared to be co-dependent on the subject's mental set, i.e. the way the subject sees the first figure, for it may be perceived in different ways. For example, a certain figure used in this experiment could be seen as: "A letter W with a line on top" or as: "Two tops side by side".

It appeared that the subject could make himself perceive one structure or the other at will. The way he experienced the shift of the first figure to the second changed with this mental set.

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COMMUNICATIONS OF THE INTERNATIONAL UNION OF SCIENTIFIC PSYCHOLOGY

SOME IMPRESSIONS OF A VISIT TO SOVIET PSYCHOLOGISTS

BY

JEAN PIAGET

After the excellent contact made at the Montreal Congress with Soviet psychologists, particularly with Leontiev and Teplov, four Parisian psychologists (or rather four who are teaching in Paris!) were invited to visit their colleagues and the institutes of psychological research in Moscow and Leningrad. These four were Piéron, Fraisse, Zazzo and myself. Besides my personal desire for information I thought that it was the duty of the President of the International Union of Scientific Psychology to accept gratefully every occasion for scientific liaison. At the last moment Piéron was unfortunately unable to go for reasons of ill health, and we three others spent about ten days together in April in Moscow with two days in Leningrad (notably at Pavlov's famous institute about half an hour's drive out in the country).

Without emphasizing the very cordial and truly friendly welcome of our hosts (we were received most charmingly at home, not only in excellent hotels), I shall first mention three general impressions which struck us with increasing force.

The first is the importance enjoyed in Moscow by men (and women) of science, independently of their position in the party. In this connection we were very much impressed to find a certain number of our colleagues in important posts and in full scientific activity; before our trip we had been wondering what their present situation would be.

The second is the diversity of individual opinions on a great number of essential questions, such as for example, the purpose of psychology. We knew perfectly well, for instance, before our departure, of the controversy that had opposed Teplov to Leontiev in this central problem, but we intended to avoid any indiscreet allusion to it. Now one day when we were discussing with those whom we called the "five greats" (Leontiev, Teplov, Rubinstein, Luria and Smirnov) the question of psychological objectives, Teplov declared, turning with a smile to Leontiev, who also was smiling, that he maintained his position one hundred percent, according to which the states of consciousness (images, intellectual operations, language, etc., in so far as *conscious*) constitute the most important aspect of these psychological objectives. When we asked them whether they believed in animal psychology, the five burst into laughter, replying that there were five different opinions on that point! And nevertheless they made up an excellent team.

Our third general impression relates to the objectivity and frankness of our colleagues on the questions we submitted to them for discussion. At the end of our stay for instance, we had a frank discussion of a certain number of questions which remained in our minds, especially concerning the real implications of

reflexological explanations in psychology: now, we found not only a full comprehension of our questions, but in addition a critical position more highly nuanced than we would have thought from some of the publications on the subject of these essential problems. We learned inter alia that the publications of Ivanov-Smolenski by no means carry authority in Moscow, as is supposed outside the country. A fine example of objectivity was furnished us by Professor F. P. Maiorov at the Pavlov Institute, where we were shown certain experiments on the "dynamic stereotypes" of chimpanzees. Whether because of excitement due to our presence or to some quite different cause, two of the three chimpanzees examined in our presence (after about 200 previous daily sessions) failed to react according to expectations. For the first time they pushed a key through the opening of an apparatus until then ignored (as a consequence of their stereotype): Now, instead of cutting short this situation which many laboratory directors would have found unpleasant, Maiorov kept us there for about two hours so that we could get a complete idea of the phenomenon that had just occurred!

In quite another field as well I noted the same effort at objectivity shown by my Soviet colleagues at the Moscow Academy of Science, in connection with an epistemological discussion, a brief resume of which follows: — The philosopher Kedrov opened the debate: "For us the object exists before our knowledge of it. Are you of the same opinion?" I replied: "As a psychologist, I think that the subject knows an object only in acting upon it and transforming it somewhat. Thus I do not know what the object is before knowledge of it." Rubinstein then proposed this conciliatory formula: "The object is a part of the world, which could doubtless be divided up into objects in different ways. Do you agree then that the world exists before knowledge?" I replied: "As a psychologist, I think that knowledge supposes an activity of the brain; now the brain is a part of the organism, which is itself a part of the world . . . so, I agree." After this followed a little discussion in Russian of which I unfortunately understood only two words: "Piaget" and "idealism". When I asked the connection between these two words, they answered: "Piaget is not an idealist." I do not think our amiable colleagues will be bound forever by this conclusion, but I warmly appreciate this attitude of reciprocity and search for agreement in a field where dissension is much more common.

I come now to some more specific impressions. But here I would need about fifty pages to mention them all, so rich and varied were the research projects that we were shown.

First I must mention the large number of Moscow psychologists. In my ignorance I had expected to find a lot of physiologists and few psychologists at Moscow. Now, although there are a great many physiologists, the psychologists are no less numerous; those of the University, those of the institutes dependent on the Academy of Sciences, those of the psychological institute connected with the Academy of Pedagogic Sciences (director, Smirnov), those of the Institute of Defectology, etc. I dare not give a number, but in a general closing session where I had the honor of giving a lecture before the Moscow psychologists (and when Fraisse and Zazzo also spoke of their work), a large amphitheatre was filled without the inclusion of any students except a few advanced postgraduates. In connection with the discussion that followed the lecture, I should like to mention

that these psychologists read everything that appears abroad, particularly in French and English. I was particularly amazed to find Russian colleagues who had read brief articles whose contents I had partially forgotten. If I may be permitted a critical reflexion, the only one that occurred to me in the course of my visit or in analyzing certain studies like those of Kostiouk on the psychology of number in children, — I am under the impression that certain writers quote non-Russians less than they read them.

If we turn now to the experiments or important research that interested us, they are quite innumerable.

In the purely reflexological field, the work of Asratyan on the diverse combinations between the tonic and phasic reflexes made a big impression on us. This research was already presented at the Montreal Congress, but we were struck by the elegance of the experiments showing the difference in reaction of animals according to attitude, and by the lively realization on the part of this author of the problems still to be resolved in the explanation of the central notions of reflexology (beginning with the notion of excitation!).

In the field of the psychology of intelligence and of thought we saw a large number of research projects on intellectual operations, especially on the solution of problems on the basis of acquired experience, on the assimilation of classroom information (Mentchinskaja), etc., but I would mention particularly the hypothesis that absolute lack of knowledge does not exist, and that new information is always grafted on previous information.

In connection with intelligence, we know that the Soviet psychologists are supposed to be distrustful of tests and statistics and to prefer direct observation or clinical observation in a natural environment. However, although they do not want "blind tests" or those made haphazardly, they have no objection whatsoever to standardized tests when there is exact verification, and they make use of correlations, etc., when necessary. "What we cannot admit, however", they told us, "is that mathematical treatment can give psychological meaning to facts which do not have such meaning."

In child psychology, great attention is given to the difference in reactions in play context and in other contexts; for instance, visual acuity is not the same in play and in the laboratory, and perceptual constants are better on real objects with motivation than on neutral figures.

The role of language is studied in the classification and perception of colors, in perceptive organization, in the regulation of motricity (Luria) and the formation of mental images (Chemjakin) in conjunction with the action of motricity. From the point of view of the role of motricity in the constitution of images, an interesting experiment of Mme Zemtsova shows that drawings of the blind are much better when their eyes are open than when closed (with electromyographic control), the situation being naturally quite different with those born blind.

In the realm of drawing I could mention numerous research projects, for instance those of Ignatiev on drawing in stages (with carbon papers changed at fixed intervals), to judge the degree of synthesis and the progressive differentiation of the parts in relation to the whole.

A great amount of energy is expended at the Institute of Defectology on the study of oligophrenia, notably by means of the EEG techniques. True oligophrenes

seem to have a different rhythm from those with organic brain defects from the very first month. In this connection, we were impressed by the excellence of the electric equipment of the laboratories, with apparatus of local manufacture.

I could add indefinitely to this list. From the point of view of the practical and immediate conclusions to be drawn from our visit, I should like to insist on two points. We made an effort to persuade our Soviet colleagues to admit how useful it would be for the psychologists of the whole world if their reviews, scientific journals and monographs contained summaries or resumes in English, French or German, so that we could see at a glance the subject discussed, and then make use of translators and more extensive analyses in the case of important results. In the second place I recalled to those of our colleagues who were at Montreal and to our new friends whom we met in the course of our visit, how anxious the International Union of Scientific Psychology is to be universal; we hope with all our heart that the contacts initiated at the last international congress of psychology and renewed in such an encouraging manner in the course of visits like ours, would result in permanent relationship and fruitful exchanges for the greater good of psychology.

VOLUME ON "PERSPECTIVES IN PERSONALITY THEORY"

One of the more tangible products of the Fourteenth International Congress of Psychology is the forthcoming volume on "Perspectives in Personality Theory", edited by Henry P. David (Wayne, Detroit) and Helmut von Bracken (Marburg), and scheduled for publication by Basic Books, New York, in the fall of 1956. Conceived during the week of the Congress, it came into existence through the cooperation of the participants in the Symposium on European Characterology, as well as several other invited contributors.

As noted in the attached Table of Contents, the volume surveys the current psychological scene in varied lands, presents perspectives on theory and method, and concludes with some critical commentaries. The whole effort has been well integrated by Allport in his Introduction, "European and American Theories of Personality".

The contributors have agreed that all royalties (if any) will be paid directly to Noël Mailloux, Treasurer to the International Union of Scientific Psychology, for the express purpose of furthering communication and cooperation among psychologists everywhere. In this sense, the volume will be the first one to be published under the auspices of the Union. Plans for translation and publication of a German language edition are under consideration.

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INTERNATIONAL DIRECTORY

An International Directory of Psychologists is being prepared by a Committee of the Division of Anthropology and Psychology of the National Research Council (U.S.A.) under the chairmanship of Edwin G. Boring, Harvard University, and with the cooperation of the IUSP.

The Committee consists of Boring; Lee Cronbach, University of Illinois; Frank Geldard, University of Virginia; Jane Hildreth, American Psychological Association; Otto Klineberg, Columbia University; H. S. Langfeld, Princeton University; R. B. MacLeod, Cornell University; and E.B. Newman, Harvard University.

The International Directory is intended to facilitate the exchange of scholarly communications among psychologists of the world. It will contain brief bio-

graphical entries for psychologists from all countries with the exception of the United States. The Directory prepared by the American Psychological Association is considered to be readily available and complementary.

Eugene Jacobson, Michigan State University, has been appointed Editor and H. C. J. Duijker, University of Amsterdam, is European Co-Editor. An editorial office has been established at the Institute for Social Research at the University of Michigan, in Ann Arbor, and at the Psychological Laboratory, University of Amsterdam.

Mailing of questionnaires to potential Directory listings was begun in the winter of 1955-56. Work on the Directory will continue during 1956, with a planned publication date in the spring of 1957.

OFFICERS OF MEMBER SOCIETIES

The following societies have furnished relevant information to the office of the Secretary-General:

Danish psychological association

President: Poul W. Perch, Buddingevej 21B, Lyngby.
Vice-President: Franz From, Azaleavej 28, Valby.
Editor of the Association's news sheet: P. V. Bentsen, Stenmarken 20, Søborg.

Finnish psychological association

President: Erkki Mielonen, Antti Tamminen. Työterveyslaitos, Huopalahdenkatu 1, Helsinki.
Secretary: Rauno Matikainen, Teollisuuden, Tyonjohto-opisto, Pohjoiskaari 34, Lauttasaari, Helsinki.
Treasurer: Erik Sylvin, Rautateiden Psykoteknillinen Laboratorio, Rautatieasema, Helsinki.
Editor of the Association's news sheet: Liisa Routio, Ammatinvalinnanohjaus, Unioninkatu 20, Helsinki.
Please send all correspondence to: Psykologiska inrättningen, Helsingfors University, Fabiansgatan 33, Helsingfors.

French society of psychology

President: Jean Stoetzel.
Vice-President: M. Delay.
Secretary-General: Paul Fraisse.
Treasurer: Maurice Reuchlin.

Icelandic psychological association

President: Broddi Johannesson, Marargata 7, Reykjavik.
Committee Members: Simon J. Agustsson, Oddagata 12, Reykjavik;
Kristinn Bjørnsson, Basenda 2, Reykjavik.

Israel psychological association

President: E. Arnstein, P.O. Box 1406, Jerusalem, Israel.
Secretary: Mrs. Y. Flum, P.O. Box 1406, Jerusalem, Israel.

ITALIAN SOCIETY OF PSYCHOLOGY

President: Mario Ponzo, Citta Universitaria, Rome.
Vice-President: Cesare Musatti, Institute of Psychology, Milan's State University, Via Pascoli 53, Milan.
Secretary: Alberto Marzi, Institute of Psychology, University of Florence, Via Cesare Battisti 4, Florence.

JUGOSLAV PSYCHOLOGICAL ASSOCIATION

President: Ramiro Bujas, Marulicev trg 19, Zagreb.
Secretary: Borislav Petz, Gajdekova 16, Zagreb.
Treasurer: Dora Vinski, Nemciceva 2, Zagreb.

NETHERLANDS PSYCHOLOGICAL ASSOCIATION

President: H. J. F. W. Brugmans, Hondsruglaan 25, Groningen.
Secretary: B. Stokvis, 1e Weteringplantsoen 2c, Amsterdam.
Treasurer: H. Y. Groenewegen, Oranjelaan 7a, Hilversum.
Representative in the IUSP: H. C. J. Duijker, University of Amsterdam.

NORWEGIAN PSYCHOLOGICAL ASSOCIATION

President: Arvid Ås, Rektorhaugen 19, Oslo.
Secretary: Eiliv Solum, Skolepsykologisk kontor, Herslebs gate 76, Oslo.
Treasurer: Bjørn Roksand, Arbeidspsykologisk Institutt, Akers gata 55, Oslo.
Please send all correspondence to: Psykologisk Institutt, Karl Johans gate 47, Oslo.

SWEDISH PSYCHOLOGICAL ASSOCIATION

President: Gunnar Westerlund, University of Stockholm, Östermalmsgatan 97, Stockholm.
Vice-Presidents: Torsten Husen, Jungfrugatan 37, Stockholm Ö;
Sven-Eric Henricson, Banergatan 79, Stockholm.
Secretary: Urban Dahllöf, Lindbergsgatan 3A, Uppsala.
Treasurer: Erik H. Malmström, Lagman Eskils väg 20, Lerum.

RESEARCH COMMITTEE ACTIVITIES

Under the auspices of the Research Committee of the Union (Mailloux, Duijker and Klineberg) a very successful meeting was held on December 28-29, 1955, in Montreal. Topics discussed included (a) next steps in the study of national characteristics, (b) research on the origin and development of national stereotypes, and (c) psychological contributions to research on "peaceful cooperation".

Participants included: Karl Bernhardt, Julian Blackburn, Guy Dubreuil, George Ferguson, Ruth Hoyt, Wallace Lambert, Bernard Mailhiot, Noël Mailloux and David Solomon from Canada, and Gordon Allport, Alex Inkeles, Marie Jahoda, Daniel Levinson and Otto Klineberg from the United States. Klineberg presided, and Lambert of McGill, with the cooperation of Mailhiot of the University of Montreal, acted as rapporteur.

Duijker has agreed to prepare a critical survey of materials related to psychological characteristics of national groups, and the meeting agreed on suggestions to be sent to him regarding the contents of such a survey. Specific research plans were discussed in connection with the other two aspects of the agenda.

In connection with these, Unesco has given two contracts to the Union in order to facilitate further developments. One of these calls for "a meeting at the Headquarters of the International Union of Scientific Psychology, bringing together a group of Canadian and American psychologists for a minimum of four days at which an investigation will be made of the channels through which the people of one nation get information about people of other nations; the meeting will also attempt to clarify how far national stereotypes change under the impact of direct experience with another country and will devote considerable attention to the problem of developing adequate new techniques for the study of the sources of national stereotypes." This meeting has been arranged to take place at Columbia University, New York, from May 24 to 27, 1956. Participants will include: George Ferguson and Wallace Lambert from McGill University, Noël Mailloux from the University of Montreal, Julian Blackburn of Queen's University, Muzafer Sherif from the University of Oklahoma, Daniel Levinson from Harvard University, Joseph Nuttin of Louvain, now at the University of Kansas, Alex Inkeles of the Center for Advanced Study in the Behavioral Sciences, Stanford, and Otto Klineberg, Columbia University.

Topics to be discussed are the following:

1. Explanation of the nature of the Unesco Contract, and the research plans of the Union.
2. Critical discussion of the contents of the working papers.
3. Discussion of present psychological knowledge concerning the manner in which people of one nation obtain information and misinformation about people of other nations.
4. Discussion of the effect of direct experience with nationals of other countries. Critical review of existing literature.
5. Discussion of techniques for the study of the origin (sources) of national stereotypes, with particular reference to their development in children of different ages.
6. Decision regarding which of these techniques can be successfully applied cross-nationally.
7. The setting up of a machinery for such cross-national studies to be initiated in the autumn of 1956 in several different countries.
8. Conclusions which emerge from the meeting.

The second contract asks the Union to prepare a report on the contribution which Social Psychology could make to the implementation of the Unesco project on Peaceful Cooperation. Preliminary research papers have been obtained from psychologists who are members of the Research Exchange on the Prevention of War. These papers are now being edited and integrated, and the resulting document will be sent to an international group of psychologists for their comments before being transmitted in final form to Unesco.

FUTURE MEETINGS AND CONGRESSES

The 15th International Congress of Psychology will take place in Brussels, Belgium, from July 28 to August 3, 1956, under the presidency of Albert Michotte, Director of the Psychological Laboratory of the University of Louvain. It is being

organized under the auspices of the IUSP by the Belgian Society of Psychology, of which Jean Paulus of the University of Liège is President. The Organizing Committee is under the chairmanship of René Nyssen of the University of Brussels, with Louis Delys, Director of the Psychological Services of the Belgian Army, as Secretary-General. The Chairman of the Program Committee is Joseph Nuttin of the University of Louvain, who is at present in the United States as Visiting Professor at the University of Kansas. The languages of the Congress will be French, English, German, Italian and Spanish.

The tentative list of topics in connection with which symposia are planned includes the following:

A. Methodological Problems.
   (1) Theory Construction and Interdisciplinary Approaches in the Behavioral Sciences.
   (2) Information Theory and Its Psychological Applications.
   (3) Phenomenology in the Description of Behavior.
   (4) Scaling Methods and Non-parametric Statistics.

B. Biological Processes and Behavior.

C. Perception.
   (1) Neurological Bases of Perception.
   (2) Perception of Space.
   (3) Perception of Time.
   (4) From Perception to Intelligence.
   (5) Dynamic Aspects of Perception.
   (6) Social Perception.

D. Psychology of Language.

E. Personality and Motivation.
   (1) Learning and Personality.
   (2) Specific Aspects of Motivation in Man.
   (3) Present Conceptions of Emotion.
   (4) Personality and Expression.
   (5) The Longitudinal Study of the Child and the Adolescent.
   (6) Infant Experience and Personality Development.
   (7) Experimental Investigations of Psychoanalytic Concepts.
   (8) Theoretical Foundations of Projective Techniques and Personality Tests.

F. Religious Psychology.

G. Problems in Social Psychology.
   (1) National Characteristics and Stereotypes.
   (2) Psychological Problems of Emigration and Immigration.
   (3) Psychological and Social Aspects of Automation.
   (4) Psychological Problems Relating to Films.

H. Cultural Anthropology with special reference to African Negroes.

I. Animal Ethology and Psychology.

Any further information regarding the Congress may be obtained by writing to: M. Louis Delys, 296, avenue des Sept-Bonniers, Forest-Brussels, Belgium.

The next Annual Meeting of the World Federation for Mental Health will take place in West Berlin, Germany, from August 12 to 17, 1956, at the Free University of Berlin. The general theme will be "Mental Health in Home and School", and various aspects of this theme will be discussed in plenary sessions, technical sections and discussion groups. The languages of the meeting will be German, English and French. Details may be obtained from Esther M. Thornton, Secretary-General of the WFMH, 19 Manchester Street, London, W 1, England.

An International Congress on Cybernetics will be held in Namur, Belgium, from June 26 to 29, 1956, with the patronage of the Ministry of Education and of Unesco, under the honorary presidency of the Governor of the Province of Namur, and the presidency of Professor Georges R. Boulanger of the Polytechnic Faculty of Mons and of Brussels University. The agenda will be divided into four sections, as follows:

Section I: Principles and Methods of Cybernetics.
Chairman: Prof. Pierre Auger, of the Sorbonne, Director of the Natural Sciences Department of Unesco.
Information theory. — Servo-systems. — Learning. — Goal-seeking. — Operational research. — Theory of Games. — etc.

Section II: Semantic Machines.
Chairman: Dr. Louis Couffignal, Inspector-General of Education, Director of the Automatic Computation Laboratory, Institut Blaise Pascal, Paris.
Computing machines. — Translating machines (language theory, programming, special machines). — Multipurpose logical machines. — Learning machines. — Imaginative machines. — etc.

Section III: Automation.
Chairman: Professor Georges R. Boulanger, of the Polytechnic Faculty of Mons and of Brussels University, President of the Congress.
Automatic machines (programmed, reflex or self-regulating, adaptive). — Automatization on the industrial scale (Automation): technical, economic, social aspects. — Automatic factories. — etc.

Section IV: Cybernetics and Life.
Chairman: Dr. W. Grey Walter, Director of the Physiological Department, Burden Neurological Institute, Bristol.
The Impact of Cybernetics on Biology, Physiology, Medicine, Psychology, Social Sciences, etc.

The languages of the Congress will be French and English. Further information may be obtained from the Secretariat, 13 rue Basse-Marcelle, Namur, Belgium.

The 4th Session of the Association of French-Speaking Psychologists (Association de Psychologie Scientifique de Langue Française) will take place in Strasbourg, France, on October 5-6-7, 1956. The theme will be "Conditioning in Relation to Learning". On the morning of October 6, there will be a report by Professor Leontiev of Moscow on "Russian Investigations of Conditioning since Pavlov".

The 4th of October is being reserved for the International Union of Biology, which

will present a program on Animal Psychology, under the chairmanship of Henri Piéron. On this occasion the Section on Experimental Psychology and Animal Behavior in the Division of Biology of the International Union of Biological Sciences will be organized.

Further information regarding these meetings can be obtained from: Paul Fraisse, Institut de Psychologie, rue Jacques, Paris 5, France.

The 4th Scandinavian Psychological Congress will be held in Copenhagen, Denmark, from June 25-30, 1956. Psychologists from all the Northern Countries are expected to participate. Further information may be obtained from the Secretary-General of the Congress: Major B. Borking, Militaer Psykologisk Arbejdsgruppe, Frederiksberg Slot, Copenhagen.

CONGRESSES AND MEETINGS HELD

The Third Interamerican Congress of Psychology was sponsored by The Interamerican Society of Psychology, and was held at the University of Texas, Austin, Texas, from December 16-21, 1955. The central theme of the Congress: *The Psychology of Social Tensions,* was discussed in various panels, such as: "The Human Relations Laboratory for Investigations of Social Tensions", — "Tension-Mapping: the Psycho-social World of the School System", — "Approaches to Intergroup and International Understanding", — "Intercultural Tensions in Europe, the Near East and North America", — "Intercultural Tensions in Mexico and the United States", — "Tensions in Childhood". Several individual papers presented in the General Sessions dealt with tension problems in communities and in psychotherapy.

Among the delegates were representatives from Argentina, Canada, the Dominican Republic, Jamaica, Mexico, Panama, the U.S.A., and Venezuela. The Anti-Defamation League of B'nai B'rith, the Hogg Foundation for Mental Hygiene, and the University of Texas gave generous financial support to the Congress Committee that offered meals, teas, receptions and entertainment to all registered participants, as well as hotel accommodations to the delegates. The U.S. State Department facilitated the travel of some of the delegates. Dr. J. Manuel Espinosa, Chief of Professional Activities of the State Department's International Exchange Program, gave the keynote address.

Contributions, the translation of which was transmitted by electronic equipment, will be published in monographs of the Hogg Foundation.

Officers of the Interamerican Society of Psychology elected for 1956, are: *President:* Otto Klineberg, Ph.D., Columbia University; *Vice-President:* Guillermo Davila, M.D., National University of Mexico; *Secretary-General:* Werner Wolff, Ph.D., Bard College; *Treasurer:* Gustave M. Gilbert, Ph.D., Michigan State University.

The Interamerican Society of Psychology plans, with its Committees on Interamerican Cooperation (Chairman: H. H. Anderson), on Membership (Chairman: S. Pearlman), on Publications (Chairman: L. Sontag), on Research (Chairman: K. Dallenbach), to further the development of interamerican psychology and education. The first monograph of the Society: "Present Day Psychology in the Americas",

written by the Society's representatives in North and Latin America, has already been issued.

Arrangements are now being made for the next Annual Meeting, which will probably take place at the University of Puerto Rico in December of 1956. Further information may be obtained from the Secretary-General, Dr. Werner Wolff, Bard College, Annandale-on-Hudson, New York.

The Eleventh National Congress of Italian Psychologists was held in Milan from April 7-11, 1956.

Among the meetings of interest to psychologists which took place in France during the past year, the following may be noted:

1. The International Congress of Filmology, organized by the Institute of Filmology of the University of Paris, met at the Sorbonne from February 19-23, 1955, under the chairmanship of M. Mario Roques, member of the Institute.

2. A meeting on Infantile Neuropsychiatry took place at the Hospital of Salpétrière in Paris on February 18 and 19, 1955, under the chairmanship of Professor Heuyer.

3. Two meetings on Dyslexia were held on the 25th and 26th of June, 1955, in the Laboratory of Hygiene of the Faculty of Medicine in Paris.

4. A Conference on Neurophysiology took place at the Salpétrière, organized by the Clinic on Diseases of the Nervous System of the Faculty of Medicine in Paris, from October 11-15, 1955. It was devoted to a Colloquium on "Activities and Functions of the Occipital Lobe". On October 13, there was a meeting of the French Section of the International League Against Epilepsy. Speakers at these meetings included: Miss E. Crossby (Ann Arbor, U.S.A.); MM. Th. Alajouanine (Paris), H. Piéron (Paris), P. Buser (Paris), P. Pichot (Paris), A. Dubois Poulsen (Paris), M. Magis (Paris), M. Critchley (London), P. Castaigne (Paris), H. Gastaut (Marseille), Lennox, P. Morax (Paris), H. Heacaen (Paris), F. Lhermitte (Paris), J. Lhermitte (Paris), A. Remond (Paris), J. Ajuriaguerra (Paris).

5. During the week of October 18-22, 1955, there took place at the Sorbonne in Paris an International Congress, held under the joint auspices of the French Association for the Study of Phonation and Language (the Fourth Annual Congress) and the Haut Collège International for the Psycho-physiological and Psycho-pathological Study of Languages and Thought (First Biennial Congress).

6. From November 7-12, 1955, there was held in Marseille a Colloquium on "Electrical Activity of the Brain in Relation to Psychological Phenomena"; the Honorary President was Professor G. Berger, Director General of Higher Education, and the Chairman was M. A. Fessard, Professor at the Collège de France. Papers were read by: G. and J. Verdeaux and R. Frances (Paris); P. Dell and G. Lairy-Bounes (Paris); N. Yoshii (Osaka); V. Rusinov and G. Smirnov (Moscow); A. Jus (Lodz); C. Popov (Paris); F. Morrell (Minneapolis); M. Jouvet and Hernandez-Peon (Los Angeles); P. Buser (Paris); J. Faure (Bordeaux); J. Schneider (Colmar); A. Remond and N. Lesevre (Paris); H. Fischgold, G. Arfel, P. Mathis and H. Torubia (Paris); C. Dreyfus-Brisac, C. Blanc, M. Samson and M. Monod (Paris); S. Dongier and Y. Gastaut (Marseille); A. Mundy-Castle (Johannesburg); W. Grey Walter and J. Shipton (Bristol); M. Kennard (Vancouver); W. Storm Van Leeuwen (Leiden); D. Bekkering, A. Kamp, J. de Lauge and J.

Werre (Leiden); S. L. Sherwood (London); D. Hill and D. Pond (London); N. Das and H. Gastaut (Marseille); A. Roger (Marseille); J. Bert, P. Gallais and H. Gastaut (Marseille); P. Picard, P. Navarranne, P. Laboureur, C. Grousset and C. Jest (Toulon); H. Collomb, G. Miletto, G. Gardaire and J. Blanc-Garin (Marseille); M. and S. Dongier and A. Angel-Villegas (Marseille).

7. On the 27th of November, 1955, a meeting was held at Rennes on the general subject of Infantile Neuropathology. The theme of the meeting was the "Exploration of Activity in Infantile Neuropsychopathology".

The Annual General Meeting of the British Psychological Society was held in Manchester from April 5-9, 1956.

Among recent meetings of the British Psychological Society, the following may be mentioned:

1. Education Section: January 28, 1956, "Testing the Very Young", Dr. Ruth Griffiths.
2. Scottish Branch: January 28, 1956, "Psychology in Aviation Medicine", Dr. D. C. Fraser, and "The Mother-Child Relationship in Schizophrenia", A. McGhie.
3. Industrial Section: February 3, 1956, "The Effective Use of Manpower", Alec Rodger.
4. Social Psychology Section: February 10, 1956, "Some Psychological Implications of an Anthropological Community Study in England", C. Slaughter.
5. Northern Branch: February 11, 1956, "The Policy and Programme of the National Foundation for Educational Research", Alfred Yates, and "Description of the Manchester Electronic Computing Machine: its application to statistical problems", Dr. D. C. Gillis.
6. South-West of England Branch: February 11, 1956, "Classroom Thinking and Learning in terms of some Contemporary Ideas in Psychology", Dr. E. A. Peel.
7. Joint Meeting of Education and Industrial Sections: February 15, 1956, "Technical Recommendations of the United States Committee on Test Standards", Dr. L. J. Cronbach.
8. General Meeting: February 18, 1956, "Exploring in Group Discussions the Factors Affecting Scientific Judgment", Dr. M. L. Johnson.
9. Education Section: February 24, 1956, "Selection for Secondary Schools — Some Follow-up Studies", Alfred Yates.

The Madras Psychological Society held its Third Annual Conference in December, 1955, in Madras, India. The Presidential Address was delivered by Dr. G. D. Boaz, Professor of Psychology at the University of Madras.

A meeting of Commonwealth Service Psychologists was held at the Defence Research Medical Laboratories at Toronto, Canada, under the auspices of the Commonwealth Advisory Committee for Defence Science, during the week of 30th January to 4th February, 1956. Delegates from the United Kingdom, Canada,

Australia, South Africa and India attended. A number of observers from the United States of America were also present.

The meeting discussed psychological matters of interest to the Armed Forces of the Commonwealth among which were: research on training, sociological factors, personality, perception and selection.

The recommendations of the meeting will be incorporated in a report to the Commonwealth Governments for their further consideration.

NEWS AND NOTES

BELGIUM

The National Academy of Sciences of the United States of America, the highest ranking scientific body in the United States, which limits its membership to 600, has appointed Albert Edouard Michotte, Professor of Psychology, University of Louvain, as a Foreign Associate.

FRANCE

Mme. J. Favez-Boutonier, who was formerly Professor of Psychology in the Faculty of Letters at Strasbourg, has been named "Maître de Conférences" in Psychology in the Faculty of Letters at Paris.

Daniel Lagache has been named to the Chair of Pathological Psychology at the University of Paris, succeeding G. Poyer, who has retired.

P. Pichot, "Chargé de Conférences" in Psychology in the Faculty of Letters at Paris, has been named "Agrégé" in Psychiatry in the Faculty of Medicine at Paris.

A Jubilee Ceremony took place on April 19, 1955, at the Psychiatric Center of Sainte Anne in Paris, in honor of the 70th birthday of Dr. E. Minkowski. On this occasion he was given an honorary Doctor's degree by the Faculty of Medicine of Zurich.

On the proposal of the Academy of Sciences, the Committee of the Institute of France awarded the Jaffé Prize to Henri Piéron, former President of the IUSP, for his scientific contributions.

A Chair of Social Psychology was created in 1955 in the Faculty of Letters at the University of Paris. Jean Stoetzel, formerly of the Faculty of Letters at the University of Bordeaux, has been named to occupy this Chair.

SCANDINAVIA

A report from E. T. Rasmussen of Copenhagen University includes the following notes:

"As you will see, there is a psychological association in each of the Northern Countries (Denmark, Finland, Iceland, Norway and Sweden). The associations' members are university-trained psychologists, and number about 800 in all.

Apart from trying to keep their members up-to-date with the developments in psychological research, the associations also take upon themselves to forward the professional interests of their members in matters concerning the authorities.

The coordination committee established by the five associations meets at least once yearly in one of the member countries. At these meetings, common problems are discussed, and it is attempted to ensure that the conditions in the Northern

Countries with regard to professional training, ethical problems, access to testing materials, wages, etc., develop in as uniform a manner as possible.

The coordination committee is furthermore responsible for the publication of a Northern periodical called "Nordisk Psykologi", which at present has a circulation of approximately 1200. In 1955, the periodical comprised 480 pages, including two Norwegian monographs and a list of the psychological periodicals available at the various libraries in the member countries. Each country appoints an editor who is responsible for the material emanating from that land. The editorial board meets once a year in order to discuss editorial policy and technical problems. The expenses connected with the periodical are defrayed by advance low-rate subscriptions and by grants the associations receive from their respective governments for this purpose.

In order to give Northern psychologists possibilities for reaching an international group of readers, close cooperation has been initiated with *Acta Psychologica,* published in Holland. It was considered undesirable to start a new periodical whose size would be limited, and preferable to coordinate work with another small European country where an international journal was already published.

The coordination committee also arranges a Northern Psychological Congress every third year. Most psychologists from the five member countries are able to speak one of the Northern languages that is understood by all their colleagues, and the official languages of the congresses are therefore Danish, Norwegian and Swedish.

The fourth of these congresses will be held in Copenhagen, June 25-30, 1956. Several hundred Northern psychologists usually attend.

Contributions to the congress include reports on completed research and professional problems. However, stress is also laid upon the presentation of information relating to work in progress, so that experiences in the various fields of psychology may be exchanged.

Thus the cooperation between the five Northern Countries in the field of psychology is very active, and because of the partial communality of language, can be further extended in the future.

It would be natural to try to develop cooperation by closer contact with other small European countries which have many problems similar to those facing the Northern Countries."

The Secretary of the Northern Psychologists' Coordination Committee is Jasper Florander, Emdrup Banke 63, Copenhagen Ø.

The Society for Philosophy and Psychology, founded in 1927, has the following officers:

President: Carl Jørgensen, M.D., Post Box 163, Copenhagen K.

Vice-President: Professor E. Tranekjaer Rasmussen, Department of Psychology, University of Copenhagen, Studiestraede 6, Copenhagen K.

REPORT OF THE XIIth CONGRESS OF THE INTERNATIONAL ASSOCIATION OF APPLIED PSYCHOLOGY

BY

C. B. FRISBY

The XIIth Congress of the Association was held from the 18th to the 23rd July, 1955, in London. It was the first time that the Association had met in the United Kingdom.

The meetings took place in the Beveridge Hall of the Senate House of the University of London, the Masaryk Hall of the School of Slavonic Studies and in the lecture theatres of Birkbeck College. The European Productivity Agency provided simultaneous interpretation facilities in the Beveridge Hall, which was used for the Plenary Sessions; this amenity was greatly appreciated by the members of the Congress.

The Organising Committee had decided to adopt a plan for the Congress along lines which experience in the United Kingdom and the United States had suggested as desirable. This involved the organisation of a number of symposia which took up rather more than half the working time of the Congress and it meant that a limit had to be imposed on the number of individual papers which could be included in the programme. The programme thus represented a considerable departure from the plan of earlier Congresses of the Association. It seemed to be the general feeling that the innovation was a successful one.

In addition to the Opening and Closing Sessions, there were four Plenary sessions held in the mornings of the week. Each of these was devoted to a symposium for which three speakers had been invited, the timetable being arranged to allow at least an hour for general discussion of the papers. The topics chosen for the four plenary sessions were:

The Psychologist and Society.
The Evaluation of Methods of Applied Psychology and the Problem of Criteria.
The Organization and Application of Research.
Attitudes, Opinions and Behaviour.

These topics were ones which it was thought would be of interest and importance to all members of the Congress and in fact a very large proportion did attend them.

In the afternoons it was necessary to hold six sessions simultaneously. Four sessions were devoted to symposia and two to individual papers, of which it was possible to accept only 40 out of the 90 offered. The symposia at the afternoon sessions were:

Motivation and Morale; Theory and Practice
Validity of Projective Techniques
Social Psychology of the Classroom

Problems of Aging
The Worker and his Tools
The Handling of Exceptional Children
Adolescent Delinquency
Technical Education and Training
Resistance to Technological Change
Measurement in Applied Psychology
Rehabilitation of the Disabled Worker
Social Implications of Industrialisation
Classification of Occupations for Vocational Guidance Purposes
Social Adjustment of the Mentally Deficient
Communication Problems

It is evident that a programme of this kind throws a much heavier burden on the organisers than does the traditional type. It also places them under the necessity of refusing many of the individual papers which are offered, firstly because there must be a limit to the number of papers given and secondly because it is desirable that individual papers should be capable of being grouped together in a logical fashion and there are, of course, advantages if many of them can be related to the themes of the symposia.

The members of the Organising Committee who planned the symposia and decided upon the acceptance of individual papers are to be congratulated on the success they achieved. The general level of the papers was certainly excellent and the programme was so arranged to provide something for each of the main groups of psychologists during every working session with a minimum of overlapping.

Several speakers besides those contributing to the symposium on the Psychologist and Society were concerned with the relationship of the psychologist to the community. The Marquess of Salisbury, the Lord President of the Council, who opened the Congress, referred to this issue. After paying tribute to the contribution that psychologists could make, he urged them to be ready to explain what they were doing in words which the man in the street could understand. "It is because the ordinary man is bewildered," he said, "that he is distrustful. He feels there must be a catch in it." The President, Dr. C. B. Frisby, also touched on it when he urged psychologists to pay more attention to the attitudes of the subjects of their research, experiment or advisory work towards themselves and to the situation in which they both found themselves.

Professor Morris Viteles, in a paper at the Closing Session entitled "The New Utopia", was also concerned about the psychologist's relation to society. It was a salutary warning to any psychologists who might overestimate the value of their contribution to social and political theory on the grounds of competence in some branch of their speciality; while not denying the need for psychologists to make value judgments on occasions, Professor Viteles reminded us that such judgments were not necessarily better than those of other men because they were made by a psychologist.